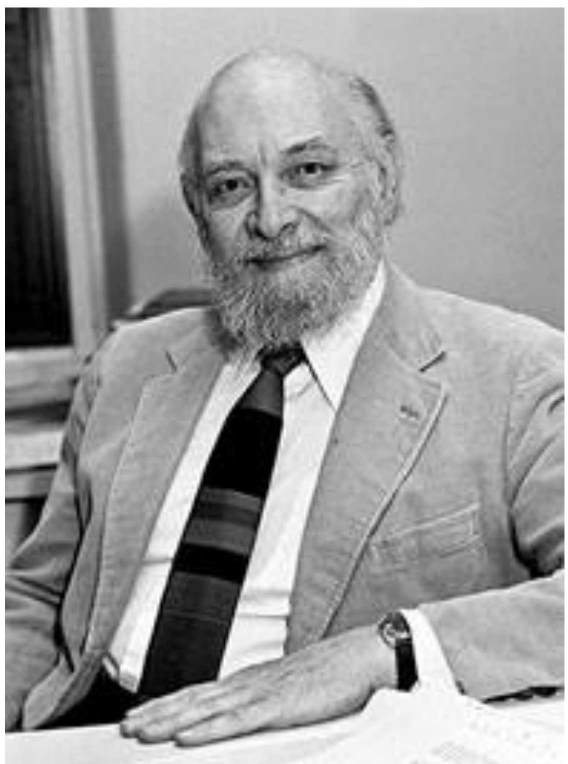
DEMOGRAPHIC DESTINIES

Interviews with Presidents of the Population Association of America

Interview with Norman B. Ryder
PAA President in 1972-73



This series of interviews with Past PAA Presidents was initiated by Anders Lunde
(PAA Historian, 1973 to 1982)
And continued by Jean van der Tak (PAA Historian, 1982 to 1994)
And then by John R. Weeks (PAA Historian, 1994 to present)
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NORMAN B. RYDER

PAA President in 1972-73 [No. 36]. Interview with Jean van der Tak at the Office of Population Research, Princeton University, May 11, 1988.

CAREER HIGHLIGHTS: Norman Ryder was born in 1923 in Hamilton, Ontario, where he also grew up. He is one of PAA's three Canadian-born presidents, along with Ronald Freedman (president in 1964-65) and Nathan Keyfitz (1970-71). He received a B.A. in general studies, with emphasis on mathematics and economics, from McMaster University in Hamilton in 1944 and the M.A. in economics from the University of Toronto in 1946. He then went as a Milbank Memorial Fund Fellow to Princeton, where he received the M.A. in economics in 1949 and Ph.D. in sociology in 1951. He returned to Canada for four years to work with the Dominion Bureau of Statistics in Ottawa and on the faculty of the University of Toronto before joining the Scripps Foundation for Research in Population Problems at Miami University in Oxford, Ohio, where he worked on the design and planning for the first Growth of American Families study of 1955. From 1956 to 1971, he was on the faculty of the department of sociology at the University of Wisconsin, where he established the Center for Demography and Ecology. Since 1971 he has been at Princeton with the department of sociology and Office of Population Research, becoming Senior Research Demographer Emeritus in 1989. He is an adviser to Statistics Canada and, among other awards, received PAA's Irene B. Taeuber Award for Excellence in Demographic Research in 1987.

Norman Ryder's insightful research and prolific writings have been focused on the analysis of fertility and family demography. He is particularly well known for his emphasis on the cohort over the period approach to studying fertility and on time patterns in fertility and family changes. With Charles Westoff, he was principal investigator in the National Fertility Studies of 1965, 1970, and 1975, which were reported in, for example, Reproduction in the United States [1965, 1971], The Contraceptive Revolution [1977], and National Fertility Study: Married Women Interviewed in 1970 and 1975 [1980], and they prepared the core questionnaire for the World Fertility Survey. [Dr. Ryder died in Princt

VDT: What led to your interest in demography?

RYDER: I was a student at a very small college in Canada--small then--named McMaster [in Hamilton, Ontario] and my professor of economics had population as his research specialty. He was the only academic in Canada with an interest in population. His name was Burton Hurd. He did most of his publications on ethnic groups in Canada, working with government statistics. He hired me to help him with the revision of a monograph on ethnic origins. He was instrumental in getting me to become a demographer because of a very specific chain of circumstances. He was on the Board of Directors of the Population Association and therefore had contact with Frank Notestein and Notestein was, of course, director of the only training program that existed then at the graduate level in demography. Notestein asked if there was a likely person in Canada as a candidate for the Milbank Memorial Fund fellowship.

Accordingly, Burton Hurd said to me, "How would you like to be a demographer?" I had to ask him what that was; the word was new to me. He described it and I found it interesting because I'd always been pretty good in arithmetic. Accordingly, he managed to have my name put forth successfully for the fellowship. And that's how I became a demographer.

Now there's a sad part to that story as well. Hurd had a particular reason for wanting to have a successor and that was that he was in very poor health and, in fact, he died three years later.

VDT: You mean he really had you in mind as his successor?

RYDER: That's right. My role was to become trained at Princeton and return to Canada to become <u>the</u> Canadian demographer, at least in the academic world. And I followed that prescription faithfully. I returned to Canada immediately after my education was completed at Princeton, after I got the Ph.D. I went to work for what was then called the Dominion Bureau of Statistics. After a year and a half there, I was on the staff of the University of Toronto for three years, and I was indeed the only academic demographer in Canada.

On the other hand, that has a lot of disadvantages. I was expected to have some kind of professional skills in diverse topics like migration, housing, labor force, education, fertility, life tables, or whatever, and it spreads one rather thin. I also had a strong urge to do research and there were literally no sources of research support in Canada whatsoever. Accordingly, when Pat Whelpton approached me with the idea of joining him at the Scripps Foundation and working on several large research projects, I decided that I was more wedded to my profession than I was to my country.

VDT: That's happened often, at least in those days, with Canadian academics. If you wanted to make your mark academically, you had to come South.

RYDER: Nathan Keyfitz and Ronald Freedman did, and a number of others. [Ronald Freedman came to the U.S. as a child.] It's sad for Canada.

VDT: It is; I hope it's improving. I graduated in history from your University of Toronto and the top people in our department all got good scholarships here at Princeton or at Harvard and so on. Well, the migration still continues. What was your thesis topic?

RYDER: I chose as my thesis the concept of the cohort [<u>The Cohort Approach</u>, Ph.D. dissertation, Princeton, 1951]. It was at that time a subject of some interest and curiosity, particularly in the study of net reproduction rates. I found a number of lengthy series, particularly one for Sweden, which could be converted into cohort form and I turned out what was not so much a dissertation as a collection of essays on the wonders of the cohort approach. I never thought of publishing the work, because it was clearly a methodology in progress [but it was published by Arno Press in 1980]. I wrote up a bibliography for many years and found that the cohort approach was well known in history, psychology, political science, and so on and that, on the other hand, only the demographers had really attempted to seize hold of the technicalities of it. I guess I've spent my whole professional lifetime as a salesman for the cohort approach.

VDT: You have indeed. You wrote already of the parity progression ratio in 1951, your first publication of that. Was that part of your thesis?

RYDER: That was in my thesis. It had originally been a paper that I gave at the Population Association meetings, in which I talked about Canadian cohort fertility and used the parity progression ratio as a particular example of how to analyze data that had that richness of detail ["Long-run and Short-run Changes in Canadian Family Size," presented at the 1951 meeting in Chapel Hill].

I invented the idea as a graduate student here at Princeton. I remember the first time I announced this idea. I said that it is not really a ratio; it's really a probability. One of my fellow students said, "Norm, you're not going to impose on us a PPP, are you?" And that's why it became the PPR.

VDT: Interesting! But then two years later Louis Henry came out with something [in <u>Fecondite des</u>

Marriages: Nouvelle Methode de Mesure, 1953]. Had he copied you or was it independent? And his was called a probabilite, wasn't it?

RYDER: He called his a probability; it was "probabilite d'agrandissement." But it was exactly the same thing, and I am quite sure that I didn't borrow from him and he didn't borrow from me. I like to think that in this way I was a little bit in the class of Louis Henry.

VDT: Indeed, you were. I recently reread an article by William Petersen on doing his Dictionary of Demography ["Thoughts on Writing a Dictionary of Demography," Population and Development Review, December 1983]. He points out that Roland Pressat in his dictionary of demography [Dictionnaire de Demographie, 1979]--I've heard this from others too--listed [included short biographies of] only 19 persons for the development of demography and the only four non-European ones were yourself, P.K. Whelpton, Alfred Lotka, and Robert Potter [and Louis Henry among the 15 others]. You know that, probably.

RYDER: No, I didn't know that.

VDT: You didn't! I remember Leon Bouvier racing in--he was to review it [English edition, edited by Christopher Wilson, published 1985]--saying, "Look at this, only 19 demographers!" He didn't particularly point you out, but that's true--you are on that list. These are people who were important in the development of demography ["associated with the origin of concepts, methods of measurement, etc."]. It must be your cohort work. [Petersen was referring to the list in Pressat's original French-language dictionary, published in 1979. The list in the English edition of 1985, edited by Wilson, may be different; Bouvier's review in Population Today mentions that even P.K. Whelpton does not appear in the biographical section of that edition.]

RYDER: I guess so.

VDT: A lot of others were left out, including your distinguished colleagues here. You are indeed known for that. How did you come upon that idea, the importance of cohort over period fertility?

RYDER: The idea came from the assignment of an article by Frank Notestein in his graduate course. It was by Wade Hampton Frost and was on tuberculosis mortality in England and Wales. Wade Hampton Frost had shown that if you take a table of death rates by age for tuberculosis and put them on a diagonal, a complex pattern becomes a very simple one. I thought that was like money for nothing; it was just a magnificent example of how a point of view can clarify an otherwise muddy scene. And indeed the concept did have a very vigorous lifetime within the study of tuberculosis and it made theorists of tuberculosis think very differently about their ideas.

VDT: The original concept had come from the study of tuberculosis; you just borrowed the idea?

RYDER: No, the original cohort article that inspired me was on tuberculosis mortality. And one of the problems in doing my thesis on the cohort approach was that it was very hard to avoid stumbling over people in other fields who had a similar perspective and insight from the cohort approach. There was no way to draw a boundary around the possibilities. You can find a rich discussion in literary history. There's a school of French demographers associated with people like Maurice Bloch and Henri Pirenne who are enthusiastic for what they call generations, which is exactly the same thing. There was the Spanish philosopher Ortega y Gasset who created a whole body of philosophy focused on the cohort. He called it the most important concept in history.

VDT: Well, it certainly has been seminal in the study of fertility. You were well known for that early on. For instance, you wrote the chapter on fertility in Hauser and Duncan, their landmark Study of Population; that was already in 1959.

RYDER: That was an exciting assignment. I'm quite sure that several others must have been asked before me, because I was just a kid at the time.

VDT: Indeed, you were early in your career, but you must have made your mark already in fertility. Now, you've already answered one of my main questions, which was what led to your particular interest in the measurement of fertility. Which, of course, you have shown brilliantly is the cohort system. I most recently read, last night, your study of cohort fertility in the U.S. ["Observations on the History of Cohort Fertility in the United States," Population and Development Review, December 1986], the history from the birth cohort of 1867 right up to the present, and you've shown that if it weren't for unintended fertility, the population would long ago have faded away.

RYDER: Let me tell you a little story which is a part of the history of the Population Association that has not been completely explained in the vignettes that you are responsible for ["Vignettes of PAA History," in PAA Affairs]. Back in the early 1960s, I had been at Wisconsin for a few years and I was chairman of a building committee to get a new social science building. Once we moved into the building, there was a large social science library research room, very handsomely appointed, immediately across the hall from my office. The secretary of the department came to me and said, "If we leave that room without a name on it, we're going to lose it; the dean will steal it back from us. We've got to have a name on the door." I said, "All right, let's put the name, Center for Demography and Ecology, on it." She said, "When was it created?" "Just now." And the Center for Demography and Ecology at Wisconsin was born at that moment.

I got myself a few research funds from the National Science Foundation and hired a part-time secretary. I had absolutely no administrative skills, so it was not long thereafter that I convinced the department to hire Hal Winsborough from Duke and as soon as he arrived, I laid the mantle of director on him and he was an excellent choice for it.

The reason I'm telling this--and forgive me if I have a sort of paternal attitude toward it--is that I've just returned from Madison and that is a very thriving institution. It is large and it is successful in all sorts of empirical ways and they are a very fine bunch of people. I am very proud of my association with that organization that I started.

VDT: You can be, indeed. Why did you choose ecology--demography and ecology?

RYDER: That had reasons associated with the Midwestern tradition in demography. Although I was trained as a demographer at Princeton in a pure tradition, if you will, the tradition of demographic interest in the Midwest was heavily overlaid with the interest in human ecology. This was particularly true at Michigan and the University of Chicago. It was a style of sociological research which was macro-analytic in form and which has continued to thrive. The human ecology tradition is, I think, usually accredited to McKenzie at Michigan and was followed up vigorously by people like Amos Hawley and others. It led to a research tradition which places large emphasis on the subject of stratification, for example, and geographic distribution, segregation, and topics of that sort. I recognized that to establish a demography center within a sociology department, it was important to have strong links with non-demographic sociologists. And it seemed to me that a very important kind of link was the human ecology link.

Now, such a tradition does not exist at Princeton. But if you look at the work of the people that

are now most prominent at Wisconsin, people like Bob Hauser, Karl Taeuber, Hal Winsborough, Jim Sweet, and others, you'll find that they are very distinctive in that. Although they are certainly well-trained demographers, they are also very actively pursuing sociological questions within what is broadly called the ecological tradition. So my choice of it was a recognition of the need for breadth in that establishment in that place at that time.

VDT: It seems very logical in principle. Are the poverty people connected with that?

RYDER: There are close links, but the poverty center itself was primarily directed by economists. Hal Watts, for instance, was the head of it for a long time.

VDT: Is that where Larry Bumpass is?

RYDER: Larry Bumpass is part of the demography and ecology center. In fact, when I telephone Larry I have a little lump in my throat because his telephone number used to be mine.

VDT: Tell me about some of the other people you worked with at Wisconsin at the center that you have a paternal attitude toward.

RYDER: The group of people that were gathered at Wisconsin while I was there were primarily from an extraordinarily strong group of graduate students at the University of Michigan. It includes Hauser and Bumpass and Sweet. They have, of course, extraordinary merit in their own right, but I attribute quite a bit of the strength they brought to their mentor, who was Otis Dudley Duncan and his wife Beverly. Those two were a tower of strength in the Michigan department and had an extraordinary productivity in terms of the graduate students that they turned out. Wisconsin has, I think, become recognized as the best sociology department in the country and one of the reasons is that our recruiting sense at that time was that this was the place to go for the very best.

VDT: In sociology, of which demography . . .

RYDER: Is a very strong part. Now demography at this institution, Princeton, has not, I think, been successful in integrating itself closely within the sociology framework. But I wanted to tell you about one other thing that happened about that time when I started the demography center. I invited the Association to have its annual meeting at Madison, partly to advertise our new center, even though it was not much more than a joke at the time. It [1962 meeting] was the last meeting that the Population Association had on a modest scale. The Association membership was growing so rapidly that from then on we found ourselves in the middle of big cities in big hotels. That was, I think, the last cozy, informal Population Association meeting.

VDT: That was 1962. Where did you actually hold the meeting?

RYDER: We had a new center that had been created on the campus for small conferences and it was just big enough to hold the Population Association. We were, in fact, located in several different hotels because there was no big hotel, but the convention center provided the kind of facilities that really no hotel had. It's one of the regrets of the growing population size of the Association that we have lost, I think, something of the charm of those Gemeinschaft meetings that we used to have.

VDT: On university campuses, indeed.

RYDER: The 1962 meeting had another significance that I was certainly aware of; I'm not sure how many others were. It was the 300th anniversary of John Graunt's book [Natural and Political Observations . . . Made on the Bills of Mortality]. Demography was born in 1662. And here was 1962 and what a fine time to have a meeting at our place!

VDT: Did you have a session on John Graunt?

RYDER: We didn't have one on John Graunt [but John Durand's presidential address was on "Demography's 300th Anniversary"]. I'm afraid that the tradition of study of the history of demography has been neglected by American scholars. It's very strong in Britain, and in France, of course, but very weak in the United States.

VDT: We had the session at New Orleans just past [1988 meeting], with Kingsley Davis chairing ["Two Centuries After Malthus: The History of Demography"].

RYDER: That was notable because it was rare. Look over the programs of the PAA. Just at that time, I got engaged in another venture, and this was a thing I wanted to mention as an element of the history of the Association. I went to the Association to determine why there was no official journal other than Population Index. I found that there had been extensive discussion of this in the past and the Board of Directors turned over to me all of the minutes of the deliberations of the wisdom or folly of starting a journal. I asked them if I could have small monies for travel to go talk with people who had founded new journals and determine what was necessary to launch a journal successfully. The judgment prior to that had been, I think, excessively conservative. There was a notion that somehow or other there wouldn't be sufficient good manuscripts turned out and that we might even be a source of embarrassment to an established journal like Population Studies. Now, none of this came to pass, of course; it turned out quite the other way.

The Association decided that it was a good idea to have an official journal and I was slated to begin it at the University of Wisconsin. The university had a policy of non-support for official journals. Accordingly, I was unable to get even a spare file cabinet, let alone a half-time secretary or any other person. Reluctantly, therefore, I abandoned the idea. Because I knew it was good thing to do to start the journal, I then went to the University of Michigan and tried to talk them into taking on what is really a substantial responsibility and there was nobody available at the time to do that. Accordingly, I went to the Association and said, "It is very important to start this journal and Don Bogue of the University of Chicago is prepared to take on the responsibility," and I recommended that they do it that way.

VDT: I did not know this about the antecedents of Demography. I just assumed Don Bogue had a dream one night. It's good to know that you had the idea; that is a most important part of the history of PAA. [Ryder's part in the creation of Demography was not mentioned in Donald Bogue's "vignette" on "How Demography was Born," published in PAA Affairs, Fall 1983.]

I want to talk about your part in the important National Fertility Studies of 1965, 1970, and 1975, although that's been well documented in, for example, the series of videotape interviews with directors of U.S. fertility surveys. [This series of six interviews was originated by Barbara Foley Wilson of the National Center of Health Statistics, conducted in summer 1985, and shown at the 1986 PAA meeting. See vignette by Barbara Wilson, "Videotaped Interviews About American Fertility Surveys," PAA Affairs, Winter 1985.]

Charles Westoff told me yesterday how you and he learnt at a meeting in 1964 that the Scripps Foundation and Ron Freedman were not going to continue the Growth of American Families surveys [conducted in 1955 and 1960], so you rushed around and got money, saying these had to be continued-

-you working already at Wisconsin; Charles Westoff here at Princeton.

RYDER: There was a little detail about the very beginning that might be interesting for the record; perhaps Charlie told you this, perhaps not. We actually met and discussed this at a meeting of a small group of population geneticists and demographers who met under the auspices of General Fred Osborn for a short meeting, here at Princeton, at which we had papers in common. It was a group that is mostly closely associated now with the Journal of Social Biology. Charlie and I were at the meeting--it was around Christmastime of 1964--and the question was would there be a 1965 study. Michigan was the particular heir to it. Since Ron Freedman had been part of it [co-director of the 1955 Growth of American Families study] and the Survey Research Center [also located at Michigan] had been part of it from the beginning [conducted the fieldwork in 1955 and 1960], surely it was theirs to do if they wanted to. But it appeared that they were not going to do it, for reasons of other commitments. Accordingly, Charlie and I thought that it shouldn't be allowed to die; we should try to raise some money for it.

We went to the Population Council--not because they would be a source but they might help us find some money. At the same time they had been approached by the cancer people [National Cancer Institute], who were very interested in this new phenomenon of the oral contraceptive pill [licensed for use in the U.S. in 1960] and wanted to have some kind of denominator for various calculations that they were making. They had no idea how many women used the new pill, to put it simply. So the Cancer Institute people were prepared to put up the money. I remember we had a particularly tense meeting at which their idea was that our contract would consist of producing a tape. Once we produced the tape, we would turn it over to them and they would play games with it. But we would not operate on that level and I was delighted that the Population Council supported us. They said to the Cancer Institute people, "If these people don't do it, nobody else is available."

VDT: How did you get it done, from Christmas of 1964 when the first idea . . .

RYDER: We worked very hard.

VDT: When was it in the field?

RYDER: Fall of 1965. Another aspect of that story is that it was apparent to all concerned that it would not be politic for the Cancer Institute to be the financial auspices for a fertility inquiry; it's just not the right connection. Accordingly, they looked around--as I understand it--at Bethesda for an innocuous alternative. And the alternative they found was the National Institute of Child Health and Human Development, who were willing to put this in as an item on their budget.

VDT: You're talking now about the second National Fertility Study? NICHD was not formed until 1968. I've just interviewed Art Campbell, so I know they're celebrating their 20th anniversary.

RYDER: No, that is the Center for Population Research [of NICHD]. Forgive me for contradicting you. The National Institute of Child Health and Human Development was in place and they were willing to have the fertility study as a line item in their budget, even though they had had nothing to do with the survey that was being financed. Subsequently, NICHD, with this as a starting point, enlarged the idea into more extensive population activity. I know of very few people who are aware that it is a little bit odd in the structure of NIH for population research to be housed within Child Health and Human Development; it's not a natural. Well, that's how it happened.

VDT: From your first NFS survey. Interesting!

RYDER: Right. So, when Campbell talks to you about the operation beginning, what began was a formal organizational unit [Center for Population Research] connected with population research in NICHD.

VDT: That grew into a long and healthy connection, because that's where most of the funds come from for population research.

RYDER: That's perfectly true. There was, incidentally, some considerable negative opinion about the idea, particularly from one of the Kennedy ladies who was on the board and she objected to the idea of money intended for child health being used to prevent children from coming into the world.

VDT: Eunice Shriver.

RYDER: That's right. She made quite a bit of noise at the beginning, but she lost the argument.

VDT: Thank goodness, because then you and Charlie were able to carry on the magnificent tradition of the surveys, which, of course, led to your work on the World Fertility Survey.

RYDER: Yes. We feel most excited, Charlie and I, about the rather extraordinary compliment that was paid to us when the World Fertility Survey was in effect conceived as a replication of our study in many countries. That's a very exciting idea. And as you probably were told, we were responsible for the core questionnaire.

VDT: Yes, I didn't have to be told.

RYDER: Hundreds of people contributed, but it was our document.

VDT: And you can be very proud. That in turn led to the Contraceptive Prevalence Surveys and now the Demographic and Health Surveys--although I presume you've seen Kingsley Davis's just-published critique of the World Fertility Survey ["The World's Most Expensive Survey," Sociological Forum, Special Issue: Demography as an Interdiscipline, Fall 1987].

RYDER: Well, I published a lengthy and, I think, in tone quite critical review of the World Fertility Survey in a book review in Population and Development Review [review of John Cleland and John Hobcraft, eds., Reproductive Change in Developing Countries: Insights from the World Fertility Survey, in Population and Development Review, June 1986]. It seemed to me that there were a number of respects in which the survey could be found wanting, and I indicated there . . . I don't want to rehearse it now.

What I learned over the years--I don't know if this is something that others feel but it's certainly my opinion--is that a survey seems to me an indispensable medium for collecting certain kinds of information, but at the same time it has inherent limitations. There are also some kinds of information that it's just no good at getting at all. And that the modest yield of the World Fertility Survey from the standpoint of social and cultural analysis is not because the practitioners were inept but that the medium they were using is not well suited to do that kind of work.

VDT: You have said, for instance in the article you had on "Fertility and Family Structure" in the UN Population Bulletin [No. 15] in 1983, what you've just said, that a survey neglects institutional settings. Are you now a promoter of the participant observation process that has come to be associated with the

Caldwells?--going to live in a village and really . . .

RYDER: I have a great deal of empathy for Jack Caldwell's position. On the other hand, I think the prescription for advancing demography is somehow or other we have to clone several hundred Jack Caldwells, and it's not going to happen.

VDT: There are over half a million villages in India alone.

RYDER: That's true. No, my thoughts go in somewhat different directions. Without attempting to denigrate the line which he is so successful at and certainly deserves credit for, I think there is plenty of room for alternative approaches as well.

One of my simplest thoughts is that the World Fertility Surveys have managed to generate some very interesting questions at the macro level. We have a lot of indices now of demographic behavior for society, and what we need to do is combine those indices with a lot of other kinds of information about society. At the moment we do very little more with the analysis of societies than to give them a name, a geographic label, or perhaps carrying our analysis only one very modest step forward--we might call them Muslim populations, or something like that. Well, it's quite clear that societies differ in important respects and that there's a lot that demography can accomplish from this macro-societal approach. Almost none of it has been done.

VDT: It seems to me that some of it has been done--the anthropologists.

RYDER: The anthropologists don't like to use numbers.

VDT: That's true--well, not other than sample sizes of 35 or something. A bit more on your work. Besides pointing out the superiority of the cohort over the period approach to fertility, you have in the past said that the longitudinal survey is rather more important than cross-sectional surveys. You've already said you criticize surveys in general, but do you still feel that after your work with the 1975 National Fertility Study, which was longitudinal?

RYDER: I'm sorry but it's quite to the contrary. Our 1975 study was a follow-up of a large group of women whom we interviewed in 1970. After the conclusion of our analysis of the 1975 data, by that time the National Center for Health Statistics had in place the successor to our survey [National Survey of Family Growth, first conducted in 1973]. So there was no point in our engaging in further cross-sectional survey work; it was being done already. But the people at NICHD were very interested in our pursuing the longitudinal direction in 1980. We debated the matter, Charlie and I, over a considerable period and decided that the yield from our longitudinal study in 1975 was insufficient to justify spending some million dollars of the taxpayer's money on another round. I guess that puts us in a rather deviant position within the fraternity. I am quite confident that we could have funded that and we chose not to; we chose not to ask. It was on intellectual grounds.

I published some of the reasons for our deciding this in an article--I like the title; it's called "Where do babies come from?" That was published in a collection presented to the American Sociological Association. It ended up in a book edited by Tad [Hubert M.] Blalock [Sociological Theory and Research, 1981]. In that article I attempted to explain that there is considerable flaw in our thinking about longitudinal studies and that large payoffs can come from other directions of work. One of my main concerns is that what is longitudinal about a longitudinal study is that you follow the same individuals. But if, as is the case with me, you don't think of the individual as the center of the social universe, then that's not the kind of longitudinal work that's interesting.

VDT: That's plausible. I hadn't known about the possible 1980 study. Another thing I wanted to ask. In your book, The Contraceptive Revolution [1977], on page 349--I don't know whether you or Charlie wrote this; this has come up again in articles of yours I've read--you wrote: "Demographers generally eschew theory . . . and in style of analysis, we bypass sophisticated multivariate techniques in favor of pedestrian cross-tabulations." I don't think that's true because yours are not very pedestrian. What do you think of that? It certainly doesn't seem so from papers given at PAA; they use very sophisticated statistical techniques.

RYDER: Well, yes and no. If you look at the product that Westoff and I turned out over the course of the three National Fertility Studies, you will find virtually no attention to questions which for many people bulk very large. For instance, the question concerning the role of multicollinearity; we scarcely touched that question at all in any of our work. We have also avoided the construction of elaborate indices. You will see no use of scaling techniques or factor analysis, for example. We are working at a genuinely primitive level in the sense of statistical social analysis.

On the other hand, we have attempted to behave responsibly with respect to the measurement of demographic variables. These are questions that you do not go to a statistics book to get answers for, because the demographic variables have peculiar characteristics which call for really the building-up of a new little branch of applied statistics. That's been happening not only with us but with a number of others. We have tried to do our work on fertility surveys with a high respect for the sophistication needed to measure the dependent variable, as it were, so that in terms of social analysis, we have purposely stayed at a primitive level. But in terms of the refinement of the dependent variable, I think that we have perhaps helped to push the field ahead.

VDT: You certainly have. That explains a lot. Another question. You write so elegantly; did you have special training as a child?

RYDER: Yes, my special training was that I had a Canadian education and a Canadian education was something very, very special. I was raised in a household of people who migrated from England and the word was something that I was taught to cherish, and I cherish it still. I think it's no gift; it requires a large amount of work. But when you get it right, it's the kind of satisfaction you can't get any other way.

VDT: That also explains a lot. Many demographers write well--or perhaps have good editors, as I've found. But yours is really elegant writing.

BREAK

While we were turning the tape to the other side, Norman admitted that his office is the last holdout for smoking in this building; he's got a lovely pipe. When did the Office of Population Research become smoke-free? It's certainly become more and more of a campaign--smoke-free in all places of work.

RYDER: It became instituted more as a practice of making smokers uncomfortable than by law, but I think it was a year ago that it became institutionalized.

VDT: I have a question here about the influence of Wisconsin and of the OPR on the field. You have spoken with justifiable pride of Wisconsin; you began that center. What do you feel has been the influence of Wisconsin? You mentioned that many people are working on ecological, more sociological, questions than is the case at OPR. You hinted that here at OPR demography never

became too well integrated with the sociology department.

RYDER: The histories are quite different. At Princeton, demography began with Frank Notestein's arrival here in the mid-1930s and he was by training an economist. There was no sociology department.

VDT: His Ph.D. was actually in social statistics, according to your own excellent article on him ["Frank Wallace Notestein, 1902-1983," Population Studies, March 1984].

RYDER: The names didn't mean exactly the same at Cornell in 1927 as we think of them now. For instance, Walter Willcox, who taught Frank statistics, taught it with a large emphasis on moral philosophy, which is something you will not find in a statistics course these days. But at Princeton the demography group, particularly stimulated by the League of Nations studies of the populations of Europe and the Soviet Union and by a number of associated monographs, attracted several first-rate sociologists because they were demographers--Kingsley Davis and Wilbert Moore, for instance. And Paul Hatt came here and wrote his book on the population of Puerto Rico. Paul Hatt, who has been dead for many years, was an example of a well-known young sociologist who acquired demographic talents and used them in conducting an important early study of a developing country. Sociology at Princeton, therefore, grew as a consequence of sociologists who had come here primarily to do demographic work.

At Wisconsin, on the other hand, the tradition of sociology goes back a hundred years. It was one of the very early, very strong centers of American sociology and it had always considered that demography was something they ought to have. In fact, when I was hired at Wisconsin, I was hired in part because of the death of Tom McCormick, one of the early American demographer-sociologists. So, the traditions of the two places were quite different, and they have remained different.

In terms of my own work, I have tried very hard to make bridges between sociology and demography. I have tried to teach sociologists what virtues there are in having the kind of backbone that demography can provide you with, particularly with respect to the conceptualization of problems, the construction of appropriate models for social process. Likewise, with demographers, I've tried to prevent them from lapsing into merely actuarial and statistical exercises.

The job of social science is very, very difficult. Many of our accomplishments in demography are not just because we are just social scientists; it's because we are good at actuarial work, chartered accountancy, macro-biology, if you will. But once our questions verge on classic social science questions, we have no more potency than most of the social scientists.

And in trying to forge that link, I think that there was much more success at Wisconsin, in part because of the ecology tradition and in part because the size of the department at Wisconsin enabled us to have a comprehensive coverage of literally the whole of sociology. Whereas Princeton has always had a small-scale sociology department and many of the people have particular interests, at which they are very good, but the whole does not seem to hold together as a system of sociological training.

Now, having made this pitch for organized instruction in sociology as an important part of a demographer's training, I must confess to a considerable amount of ambivalence, because I also have the feeling that good research has not depended to a considerable extent on a person's dedication to one discipline. I think the population office here at Princeton has been a very successful one and I can assure you that months go by in which nobody identifies him or herself either as a sociologist or as an economist or as any other kind of specialist. We are here because we enjoy doing research and we do not allow disciplinary barriers to get in the way of doing good research.

VDT: Do you think demography in itself is a field--that those who have been really important in it, built it up, have been those who have no barriers between the disciplines?

RYDER: I think that we have a strong research tradition and the way it manifests itself is that if a person or persons doing a piece of work need to know something substantively in a particular area, they take the responsibility of learning that for themselves. I think the best interdisciplinary work goes on inside one person's head. I don't think you can train a person to be a comprehensive scholar; it's a much too demanding exercise. But with good research questions, if you follow where those questions lead you, you find yourself acquiring what you may need--in biology, in history, and the like--just because you are going to keep on after that question to find an answer to it.

VDT: It seems to me that many demographic research questions now, as in any field, are getting narrower and smaller; the big questions have been done. Do you think that is affecting demography too? You're saying that a good research question demands widespread approaches. Demography seems to be perhaps one little box that draws on a lot of disciplines around it. Perhaps what I'm asking is, Are there still big research questions in demography to be answered?

RYDER: One of the natural tendencies of a person who is interested in a field which is not only intellectually attractive but also has extraordinary social elements to it--it is immediately evident that our work means something to major questions that people of the world are concerned with. One of the natural things to do in an area like that it seems to me is to focus on those variables, such as population size and the level of fertility, and place them somehow or other in the center of the conceptual network that you're working with. I think that's probably been a mistake. I don't think these are the proper places around which to construct theoretical systems.

I think that if we are to advance further within fertility, for example, we're going to have to know a great deal more about the societal context out of which fertility comes and of which fertility is only a manifestation--extremely important for the development of population size, but not therefore belonging right in the center of your theoretical structure. I think it's far more important for us to be students of the family than students of fertility, as an example. And I think the same thing goes for the major questions with respect to the good or harm that is done by a particular rate of population growth. That question seems to me to be part of a much larger package of economic questions in which, I'm sure, population has a very large role to play, but one of a number of players rather than as the center of the universe.

We've done our job now and made the place of our variable stand out in the minds of others. But if we're going to advance further, I think what we're going to have to do is to enlarge our models beyond the narrowly demographic framework and tackle some questions that are much more difficult but in the long run much more rewarding.

VDT: People of the National Academy of Sciences study [Population Growth and Economic Development: Policy Questions, 1986], under Sam Preston, who is an OPR graduate, felt they were doing that when they looked at population growth and economic development and came out with answers that not everybody was happy with.

RYDER: That's exactly where I look for the future of the field. The people about whose work I am most excited are people who are well-trained demographers and at the same time have something strong going for them of a substantive sort. There are some questions that can be dealt with which are essentially little mathematical games. I love playing them myself and so do others; some of them have turned out to have rather interesting consequences. But I think the future of the field belongs to people who are prepared to be in a sense both, both a sound technical demographer and a person with genuine substantive insight. Certainly Sam Preston and Jack Caldwell fall in that category. And I think the future belongs to people of that sort.

VDT: Interesting observation; very good. Tell me about some other people who have been influences in your career. You mentioned your first Canadian professor, who obviously was of great importance; he got you here. Who did you work with when you came to OPR?

RYDER: I'd like to be able to say that I'm an intellectual heir of Frank Notestein, but that wouldn't in fact be true. I think the most important training experiences I got here were of a quite different kind. One of them certainly was the extraordinary quality of the small number of graduate students who were here at the same time as me [1946-51]. They included people like George Barclay, George Stolnitz, Robert Osborn, George Mair, Harvey Leibenstein, and a few others. We were very closely knit. We were thoroughly dedicated to demography. You could come here Sunday night at 11 o'clock and you would see lights blazing in several of the office windows. We had a high sense of camaraderie and I think we had a kind of cockiness to us because we felt that we were onto something pretty exciting. I would say to the extent that I have some influences, the influences came more from the extraordinary collection of people that happened to be assembled here at that particular time.

Now there was a second line of influence and that was that I came here as an economist and went away as a sociologist. And the reason for that was the influence of just a few people and those were, in particular, Kingsley Davis, Wilbert Moore who died last year, Marion Levy who is still here, and Paul Hatt who died 25 or 30 years ago.

In particular, I found the work of Kingsley Davis to be exceptionally stimulating. In fact, I don't think I have ever read anything of Kingsley Davis's that I haven't felt that I learned something from. He was a most remarkable intellectual influence on me. It wasn't that our time together was lengthy and we certainly haven't worked together since, but if you think that I can write well, you should read Kingsley Davis.

VDT: Yes, indeed he does. And he has shown remarkable change through all his decades of productivity--coming out decade after decade with new ideas and fresh expositions.

RYDER: I didn't want to say anything negative about Frank, on the other hand. I found him to be an extraordinary example of the importance of what you might call a "research entrepreneur." Another such example would be a man like Phil Hauser. These people seem to me to have been of extraordinary importance, extraordinary as organization creators, extraordinary as the people who managed to inspire research funds to come in our direction, people who played a major role in shaping the field just by their choice of individuals, choice of students, choices of fellow faculty members.

I had no perception. I thought somehow or other in a naive way that advances in the field came out of the head of somebody as he or she was sitting thinking hard. And it's not that at all. There's an apparatus; there's a world system. And Notestein was master in those areas. So I have unbounded respect for him, in part because he had talent that I had no way of emulating at all.

The other way round, I was unable to convince Frank of the worth of doing the research that I was doing. In the end, he sort of just let me do it; that was my doctoral dissertation. I wrote it for him, but he didn't know why I wanted to do it, what I wanted to do.

VDT: Cohort fertility?

RYDER: That didn't catch fire with Frank, no.

VDT: I didn't realize you went to work with Scripps some time after this. Everybody credits P.K. Whelpton with cohort analysis.

RYDER: I had four years in Canada. I was doing my cohort research from 1948 up through 1954, and it was because Whelpton knew that I had been doing the same kind of work that he was doing that he asked me to come down and, among other things, help him revise his cohort tables, because they were in pretty bad shape. We got the tables back into shape and then when I left Scripps, Art Campbell came and finished up the job. Those became the basis of the Heuser tables that are now published every year [in the Natality volumes of the National Center for Health Statistics].

VDT: Which you have drawn on for your good history of American fertility. Is that another case of two independent researchers onto the same thing; like you and Louis Henry both onto the parity progression ratio, you and P.K. Whelpton were independently onto cohort fertility?

RYDER: Well, if I had to look for a person who had that characteristic more than anyone else, it was John Hajnal. Hajnal was a brilliant young man who became associated with the British Royal Commission on Population [in the 1940s]. He had had none of the ordinary kind of training one would expect in the United States to produce a demographer in some sense or other. Yet, in a matter of it seemed almost months or a couple of years, he contributed mightily to the basic methodology, particularly of the cohort orientation. You can find all this in his Royal Commission paper ["Births, Marriages, and Reproductivity: England and Wales, 1938-47," Papers of the Royal Commission on Population, II, 1950]. I found it very frustrating to get an idea and then glance through some of Hajnal's stuff and find that he had exactly the same idea, but expressed with much more elegance.

VDT: Impossible. What brought you back to OPR [in 1971]--just straightforward to work more closely with Charles Westoff on the National Fertility Study?

RYDER: No, that wasn't it; it was something quite otherwise. While I was at Wisconsin, I became very much involved with the activities associated with the student uprising in connection with the Vietnam War. For a number of years, I must have spent three quarters of my time in involvement on the campus with organizations of one kind or another. My best friend at Wisconsin was Bill Sewell, the sociologist. He became the chancellor at Wisconsin and during that time, I spent a lot of my year working in a sense on his behalf, but also on behalf of the student movement, because I was one of a small number of professors whom the students continued to trust.

Events at Wisconsin took a rather familiar turn. There was a xenophobic reaction among the populace. The Board of Regents and the Governor were in the wings attempting in a heavy-handed way to make things better and in consequence making things worse. They were sending in troops with fixed bayonets and tear gas and the like; they were assembling bully-boys from the county cops. And the confrontation on campus was most unpleasant--so incongruous in an academic setting. That would be the subject of another couple of tapes.

What happened was that I became convinced by a series of actions that the higher-ups were taking at Wisconsin that they were changing the character of the university in a most negative way, until the place became a place at which I no longer wanted to work. I had been treated very well at Wisconsin and I had a deep affection for my colleagues there and have regretted ever since leaving there. On the other hand, I found that I could not, on principle, remain an employee of that university. So I gave back my chair; I resigned. I phoned Charlie Westoff and said, "Do you happen to have a job for a middle-age demographer?" And that's exactly why I came to Princeton.

VDT: I didn't know that story. Thank you for telling it. Coming to Princeton you worked with Westoff, as you had done already. How had you been operating? Sending the data back and forth from the fertility study?

RYDER: It was remarkably straightforward. I learned to write a request for tabulations that I could communicate by mail with any programmer here at Princeton, so that all of my tabulation requests were fulfilled here and stuck in the mail in those giant envelopes and sent out to Wisconsin.

Charlie and I did have a substantial division of labor. We did not work closely on piece after piece. He would so some; I would do others. And each would serve as sort of a critical editor for the work of the other person. So it was not necessary for us to have continuing conferences. I had a chunk of work and I just proceeded to do it. So it was not at all difficult from a physical standpoint to carry on research at two places. That would have been no justification for me coming to Princeton; there was no reason for that.

VDT: Tell me of some of your students both at Wisconsin and perhaps here whom you have been most pleased with--given you great satisfaction.

RYDER: That's a rather difficult question to answer because I have not had the kind of career that, say, Ansley Coale has had with respect to students. He has been most blessed, perhaps because of his characteristics, in the long list of students he has had.

VDT: Yes, he took the OPR alumni list this morning and ticked them off for me, and it went on page after page. He has had a lot.

RYDER: My list of students is a much smaller one and there are some that I'm very proud of. But I think that if I've had any substantial role as a teacher it's been that I try when I write my articles to write somewhat in a pedagogical style. And I feel very satisfied when I see that some of my things are on reading lists in different places around the world. But as a one-on-one teacher, I've had a rather small list of names.

VDT: Well, you certainly have had an influence. In the session at the PAA meeting just past in New Orleans [1988] on "Fertility Change and Its Effects on Family Structure" that you chaired--Jane Menken had organized it--the three readers I heard all said, "Now, as Professor Ryder has said . . .". They were all, I think, following your lead in doing some empirical study.

RYDER: Let me say something about that. I think part of that has to do not so much with any grand theorizing, because I really think that in the rather primitive stage of social science as a discipline, what we really need are good questions. I don't think we're really terribly near good answers. Good questions, though, can provide you with a research agenda. They say, "Here is something we need to find something about, because it's interesting and it's important."

And what I have tried to do throughout my whole career is really to lay out research agendas for people. And it's turned out in several cases that I have in a sense been able to strike oil, because I fingered a problem early on and then others gradually perceived that, yes, it was an interesting problem. For one thing, for example, I have been almost obsessed with the question of the time pattern of fertility. That was a subject on which practically nothing was known in the late 1940s when I got started as a graduate student. Bit by bit I kept hammering away at the theme; we've got to pay attention to the time dimension of the whole process because it's important. And, of course, the cohort approach has that as one of its running threads as well. I am delighted now that this has become a major industry, as it were, within the research field in demography.

Another example is the area of family demography. Now, I happen to be fortunate enough to get in on the ground floor so that I wrote several pieces, which were really prospectuses, saying, "Take a look at the gorgeous number of interesting questions that can be addressed within the framework of not individual demography, which we all know, but family demography, in which there are some

prickly questions that are of major social importance."

So I think that if I have some kind of influence that a large part of it has been in flagging good questions. I certainly haven't provided the answers to them but I have, I think, managed to indicate the crucial role that an answer would play in the larger development of our body of knowledge. It's interesting to me how fields proceed like that. Sometimes, of course, there are questions that come up on which a very large amount of money is expended and then in the end you realize it was a waste of time. Optimum population is a concept, for instance, on which a colossal amount of time has been wasted and I'm glad that it died and has been buried. But what could we have done with that talent had it been redirected a bit!

VDT: On family demography, did you ever work with Paul Glick?

RYDER: When I said that I was interested in the time dimension of fertility, there is a rich old literature in a staple part of sociology on the family life cycle, and particularly the rural family life cycle. One of the reasons was that the agricultural experimental stations in the United States all had their little rural sociology groups associated with them and it was a very active research tradition. Agriculture experiment stations have built-in research programs. Unlike the ordinary academic departments where you teach for a living, these people had a half-time research component, just like the other more practical aspects of ag experiment stations. So there was a large tradition of rural sociology, research-oriented, collecting life cycle data for farm families. This is a big literature, and it's a literature that goes back a long way in Europe as well. In the Soviet Union, for that matter, before it became the Soviet Union, there was research on farm family life cycles. Now, Paul Glick was quite clearly a sociologist from the beginning and had knowledge of this rich literature. Then, of course, he had the ability to convert it into some practical calculations, using really good data for national populations.

The whole life cycle literature has been an important one to me, in part, of course, because I have always insisted on the importance of carrying it along a cohort orientation rather than in cross-section, but partly also because it's connected with this whole time-pattern question, the time pattern of human behavior.

Paul and I have never worked together closely, but we must have been on many, many panels together because of the overlapping of our interests and the time dimension in human behavior, meaning such things as your educational career, your occupational career, labor force history, marriage, employment history, veteran's history, and so forth. All of these threads are being woven together by some people working at the University of Wisconsin in that demography center. It's become itself a major growth industry within the sociology profession, but with very strong demographic roots.

VDT: Didn't Wisconsin have a good rural sociology tradition?

RYDER: Yes, it's one of the strengths of Wisconsin.

VDT: That explains in part what you just said about family sociology developing there.

RYDER: Yes. Well, as you probably know, a lot of social research in the United States came out of a tradition of social statistics without any particular other identification to it. People who were happy to be working with numbers--the numbers were predominantly numbers that were generated out of the census and other such sources. They were sometimes called sociologists, but they were clearly quantitatively oriented, and there wasn't a clear distinction between what was demography, what was sociology, and what was even statistics. They used to call it statistics, although nowadays, of course,

statistics is the methodological apparatus rather than the numbers you're looking at.

VDT: What do you see as the important issues in demography over the years you've been involved? You never worried too much about the baby boom, did you? That has loomed large in U.S. demography. I mean that kind of thing.

RYDER: It's almost inevitable that one's thinking tends to be heavily impressed upon by the particular context in which you're studying. And in the immediate postwar days when I was studying, the preoccupation in the literature we were reading was low fertility--that was a large part of the reading list that Frank Notestein gave us--and it seemed to be a little out of date, to say the least.

I remember that early on I got concerned with an issue. How it came about was this. I was the Milbank Memorial Fund Fellow and my job was to read proofs on the Population Index. Louise Kiser was the editor here; Irene Taeuber was our editor based in Washington at the Library of Congress. Louise Kiser used to sit with me and for a while she would read and for a while I would read and we would actually read proofs on all of the papers of the Index. This required spelling out many words, because we were a multi-lingual index from the very beginning. So if you ran into a Hungarian name, you literally would spell out every letter in the name.

Louise went to Frank Notestein and said, "This is a lousy job to have a graduate student doing because he's not going to learn anything out of that; it's just scut work. Is there anything else that you could have him do?" Frank thought for a minute and said, "In the back of the Index we publish birth and death rates and things like that." Most of these came from routine sources and it was just a matter of copying them out. This was, of course, prior to the United Nations; the Population Division was in process of being created at that time. So the international sources were, for a while, the Population Index.

In the Index were published not only the routine calculations but also net reproduction rates, intrinsic rates of natural increase. These required a little calculation. Accordingly, Frank said, "Why doesn't Ryder do those calculations?" Now, I have a bit of a stubborn streak and I didn't want to do a lot of calculations unless I understood exactly what it was that I was calculating. So I started to investigate the literature on the net reproduction rate and found that there were a number of aspects in which it was defective. I think Frank was a bit wary of my radical notions with respect to the net reproduction rate, for which I had developed a substantial amount of contempt. He therefore paired me with George Stolnitz and together we thought and put together a manuscript, which became the first publication for him and for me. It was called "Recent Discussion of the Net Reproduction Rate."

It had a huge bibliography and we attempted to sort out the main threads, all of them quite critical of what had become a sort of linchpin of demography. Stuart Chase in a book called The Proper Study of Mankind--one of these popular histories of the social sciences advertising what the social sciences were for the layman, a Book-of-the-Month-Club sort of thing--said when he got to the study of population that, "The strongest single contribution that social science has ever made is the net reproduction rate." And here we had come in with a damaging indictment of the net reproduction rate. It was published in Population Index [April 1949]. Right at the beginning we had a statement that goes something like this: "Until now the questions surrounding the future growth of the population were regarded as reasonably well understood. This is now a matter of some doubt."

VDT: Oh boy!

RYDER: At that time, Joe Davis of the Food Research Institute at Stanford was attacking demographers vigorously for being charlatans because all of their forecasts were going screwy; instead of going down, everything was going up. He seized upon this particular quotation of ours and said in a number of places, "Some doubt, indeed." We thought we were being daring as graduate students to

say "some doubt," but he took it the other way round.

VDT: Right then you were seeing that population growth was speeding up in developing countries [and in the U.S.]?

RYDER: The net reproduction rate was just a misleading index, particularly because it was period-oriented. I had quite a bit of contact with Alfred J. Lotka at the time, because the index is obviously derivative of his larger stable population model. What bothered him was that people were confusing our criticism of the particular substantive application with the quality of his model. Now, his model was impeccable--and is. It's one of the great glories of scientific creation in this century. But the net reproduction rate as calculated was, and still is, badly misused. We'd be better off without it.

VDT: What would we have in its place?

RYDER: Perhaps a little thinking.

VDT: Come now, we have to have something--replacement level fertility. We've got to have that line across the chart so people can say you're above or below.

RYDER: I have been studying some recent statistics for the United States and the total fertility rate has been moving in a way that leads people to think that fertility has stopped declining and that our level is such and such and so and so. All of these are contaminated by the period orientation underlining the data. If you look at the same information from a cohort standpoint, you get a quite different story. It's just as true now as it was 40 years ago. Unfortunately, there isn't a button you can push and get one answer that answers all the questions.

VDT: You did show in your article in Population and Development Review of December 1986 that the last two birth cohorts that you looked at, which were 1955-59 and the one before that, had had cohort fertility below replacement, but a little higher than the actual period rate.

RYDER: So far they seem to be almost identical, these two adjacent cohorts. In other words, whatever has been happening appears, at the moment at least, to have come to a halt. [See Ryder's later publication on U.S. fertility trends, "What Is Going to Happen to American Fertility?", Population and Development Review, September 1990].

VDT: I guess recent women aren't going to give up entirely having children. The question was: What do you see as important issues in U.S. demography over the years you've been involved? You've just given a marvelous story of the net reproduction rate.

RYDER: We were talking about the phenomenon of the baby boom. My reaction to it was that this was a temporary divergence from a long-term trend. I always felt that demographers showed indecent haste in abandoning a long tradition of understanding of the forces underlying fertility decline just because they had a baby boom. Now, of course, in terms of practical consequences the baby boom was an enormous phenomenon. We will live with it for the rest of our lives, and our children's lives, for that matter. On the other hand, from a standpoint of more pure theory, what we really needed was a healthy dose of methodology. We had to start measuring what we were talking about instead of a poor substitute for it.

VDT: You certainly managed to do that. What accomplishments have given you the most satisfaction?

I think we've heard that--your cohort fertility, the emphasis on that.

RYDER: Let me tell you about an accomplishment that I think was a very big mark that I was able to make and I was able to make it quite fortuitously. For three years, I served as editor of the official journal in sociology, the American Sociological Review. During those years, one thing that struck me was that sociology had, in my judgment, been suffering under the burden of an extremely old-fashioned method of bibliography and footnoting. So I got together with the printer of the journal, Henry Quellmalz, and said, "Why don't we revise this and bring it more in line with scientific practice?"

So I instituted a completely new system of footnoting and bibliography citations, the one in which you give the last name of the author and the year of publication in parentheses and then at the back of the article, you read the list--names, titles, years--all the way down. That became the style not only for American Sociological Review but for all sociology journals. It became the style for all sociological dissertations, for Demography, and for journals all over the world. And this is a contribution, because I've changed the way secretaries behave all over the world!

INTERRUPTION

VDT: We've just had a visitor, which had to do with the turmoil that OPR is going through because a number of staff appointments have to be made and some people are retiring. Norman says he is technically going to retire next year.

RYDER: What I wanted to say is that retirement technically means that I will no longer be a professor and therefore I will no longer be giving a course or a seminar and will no longer be going to department meetings. Those are activities that I can live without.

As far as demography is concerned, on the other hand--it's a little hard to convey this and it may sound silly, but when I got into demography I didn't do it because I had any yearning to save the world, I got into it for very selfish reasons. It looked to me as if it had interesting problems. They are problems that I have enjoyed working on in exactly the same way that people will enjoy working on a crossword puzzle or something like that. And it has always been an extraordinary aspect of these games that I enjoyed playing that I am allowed to continue to play them, someone pays me a salary, and occasionally it makes a difference to people with respect to genuine social problems around the world. These things are fringe benefits of something that I would do even if nobody were paying me at all. So, as to what I am going to do in my retirement, I'm going to continue playing my games.

VDT: Wonderful! What a wonderful career you've had. Not many people have that opportunity. The question was, what have been some of the accomplishments that have given you the most satisfaction. You've just told of a very practical one which I'm interested to know about, the method of footnoting in sociological and other journals.

RYDER: I told you before about identifying problems that I thought were worth working on, advertising them to the profession, and then finding that they became attractive to young people and then became larger activities--this is something I got a very great deal of satisfaction out of.

VDT: You've mentioned the net reproduction rate, family demography . . .

RYDER: And the whole problem of dealing with the time dimension in demography. And dealing with the time dimension means not only adopting a cohort rather than a period orientation, it also means making a serious effort to cope with the very difficult problems of handling the succession of

events that are important in people's lives.

These events, of course, are certainly many of them demographic, but many of them are somewhat more subtle than demographers can handle very well, such as the passage of the child from the family of orientation, the family of procreation, and other such subjects--movements in and out of the labor force, transformations of households by the passage of people and the like. This area in sociology, which was quite moribund only a few years ago, is now a rich field for exploration and many people are involved at the most highly technical level and history analysis but also at what I consider to be a very important task of any social scientists and that is learning how to describe the behavior of people. It sounds pedestrian but I think that our discipline has suffered--our discipline meaning sociology--because we tried to fly before we learned how to crawl. Every science, in my judgment, has begun by a very careful attention to the details of description of what it was they were interested in. And we are only now, I think, getting around to actually describing what people's lives are like.

VDT: Are you going to write all this up, going to write your autobiography or history? You mentioned earlier that Americans do not have a good tradition of looking at the history of demography; you said the French have done it.

RYDER: I have a very close friend for whom I have a most profound admiration. He was once also president of the Population Association [1968-69]. His name is Dudley Duncan. He, unlike many demographers, played a major role within sociology as well. He has recently retired and he wrote a book about two years ago called, <u>Notes on Social Measurement</u> [1984], which was a collection of essays on the history of efforts to make measurements, with particular attention to social questions, such as how do you vote and such pedestrian kinds of questions as that. I found myself thoroughly bemused by the questions he was raising, particularly with regard to how one can go about making measurements in the social and psychological spheres.

It reminded me that when I was a graduate student I must have wasted a very large part of my time reading the history of early fumbling attempts at creating demography. That is something that I had to wean myself away from at that time because it was much too all-embracing a subject for anybody who had to write a dissertation. But I think that one of the luxuries one has in the emeritus status is to expand a little in that direction. I wouldn't be at all surprised if I spent a lot of my time reading old demographers and trying to figure out just exactly what they were trying to say.

VDT: And going on and writing something from that, I hope.

RYDER: Well, I <u>am</u> a fairly compulsive writer.

VDT: You wrote an excellent tribute, story, on Frank Notestein, which appeared in <u>Population Studies</u> in 1984, the year after his death, that was a wonderful history of the field, like Notestein's own article ["Demography in the United States: A Partial Account of the Development of the Field"] that appeared in <u>Population and Development Review</u> in December 1982, just before his death.

RYDER: One thing that has made me feel especially fortunate to be a demographer is that I was able to meet many of the major players in the beginning of modern demography as a student and as a young person. Not only Frank Notestein, but also people like Frank Lorimer and Irene Taeuber. The people who were the movers and shakers in the field were friends in the sense that I was part of the Association and the Association was a small roomful of people.

VDT: Do you remember your first PAA meeting? It obviously must have been at Princeton.

RYDER: It was at Princeton; it was in the spring of 1948.

VDT: Actually, 1948 was in Philadelphia.

RYDER: Yes, that's true. I was a graduate student at Princeton and we did go down to Philadelphia. I think that was probably where I first met Westoff, who was a graduate student under Dorothy Thomas.

VDT: He said his first meeting was 1949, at Princeton; he's not missed a single meeting since. So you remember going to Philadelphia, that was the year before. Anyway, one of those was your first one.

RYDER: You understand that we here at Princeton were the graduate students in demography; there were no others.

VDT: Okay, that's the way to put it.

RYDER: What I was going to say was that talking with Frank, I became aware that so much of what we think of as the structure of modern demography has been created in a relatively recent past so that although it's certainly well before my time, it was not before Frank's time. In fact, I think one outstanding characteristic of Frank Notestein's life was that he arrived on the scene and it was at this critical juncture in his career and in the career of demography as it was being created. There was a meshing.

I have a great feeling for the people who have made up our history. It was a very rich one and one that we can be extremely proud of, I think. The number of names who were making it possible was a finite list, a small number of people.

VDT: Indeed, it was. It's almost encompassed by those who have been president of PAA. That loses a few, like Raymond Pearl, who was never president because he died rather young.

I'm interested that you know Dudley Duncan; I hope to interview him next year [see interview of May 3, 1989]. I know he's in Santa Barbara. Do you still have contacts with him?

RYDER: Oh yes, I had a long letter from Dudley just a week ago. You should certainly interview Dudley. He is a man of extraordinary gift and is truly generous.

VDT: How did you become friends?

RYDER: He was a professor at Chicago and my contacts with Chicago when I was at Wisconsin were very close. Phil Hauser has been a friend of mine for my whole life and also Donald Bogue. Bogue and I were colleagues at Scripps Foundation when I was working for Whelpton and then Bogue left to go to the University of Chicago. I'm sure you've talked with Don Bogue.

VDT: I hope to talk to both Phil Hauser and Don Bogue [I did--see their interviews].

RYDER: Dudley Duncan was working at Chicago and, in fact, became a very potent force in demography during that time, not least because of his contribution to the volume, The Study of Population [coeditor with Philip Hauser, 1959], which as you probably know was something like a brief to the National Science Foundation to establish the fact that we were entitled to science funds because we were a legitimate science. The project was called "Demography as a Science." The

National Science Foundation during the 1950s was beginning to get strong pressure to fund something other than the physical and natural sciences. They were certainly not prepared to fund sociology across the board. Demographers, on the other hand, had something going for them by the way of mathematical underpinning and concern for empirical testing and the like that made them a sort of attractive entering wedge within the National Science Foundation for the whole of the social sciences. We prepared the brief, Demography as a Science, to the NSF, saying: "We belong. We belong to the system because we are a science." And each assignment in effect was to say where do we stand with respect to such-and-such a subject and end up with in a sense a statement of a research agenda. That was the format under which we were working.

VDT: How did Duncan and Hauser find you?

RYDER: We knew each other already. You must remember that the profession was very small; everybody knew everybody else by first name. At the meetings we had it was possible for you to say hello to everybody; no physical problems involved at that time. So we knew each other, because we were in the same business. There was nothing extraordinary about it.

Duncan trained a considerable number of people at the University of Chicago who went on to greater things. He then moved to Michigan and had an extraordinary career as a teacher at Michigan and then chose to move to Arizona. It was only in the last couple of years that he went to Santa Barbara. His wife, Beverly Duncan, was also a very important person, both in terms of research and of training. She was ailing and I think that was probably one of the reasons for their move from Michigan to Arizona. Beverly died this past winter and it was a great loss to all of us.

VDT: And Phil Hauser, you say, you've known all your life?

RYDER: Phil too was one of those people who had the talent Notestein had for being a research entrepreneur. Phil's career, of course, is a very long one. He was a "wunderkind" in the late 1930s. He was associated particularly with Henry A. Wallace when Wallace was Secretary of Commerce. Phil played therefore a very important role for a long time as a Census Bureau official, before he had this very important career at the University of Chicago, where he became an intellectual leader for the demography group at Chicago which after Princeton was the next, I think, major training center that was created in the country.

Hauser's work was in part his own personal creativity and industry but it was in part also his talent, like Frank Notestein, for finding very able people and making it possible for them to do what they could do. In that sense, I think that Dudley Duncan is something that Phil Hauser should feel very proud of, because he provided a suitable niche for Duncan to do his work when he was at Chicago.

VDT: Was Duncan a student of Hauser?

RYDER: I don't think that Duncan studied under Hauser, but he was a staff member within the organization that Hauser established for population research and training. Hauser has also played a very important role as--if you will--an ambassador to the outside world. He, unlike most demographers, is able to be at home and talk convincingly with the outside world, with the world of business and industry and commerce, and talk on major issues with some technical facility in a way that doesn't bore them silly. People like that are very important to our profession. Most of us find that role very uncomfortable and don't do it very well. Yet somehow or other the health of the profession depends on interchange between these different worlds.

VDT: Indeed, it does. You pointed out earlier that because demography deals with issues that are of

social importance it's inevitable that demographers will be wanted as spokespeople on these issues.

RYDER: That's one of the nice things about being a demographer. I don't know what people do when they have a specialized branch of biology or chemistry or whatever that they happen to do their professing in. But a demographer, on the other hand, when we talk shop everybody at a cocktail party enjoys the talk.

VDT: That's true. My sons used to say, "Oh, Mom, don't get onto population again; come off it," but they found it interesting. None of them went into it [but a Canadian niece is now, 1990-91, enrolled in the graduate demography program at Wisconsin]. Incidentally, Robert Hauser is a nephew of Phil Hauser, isn't he?

RYDER: Yes. The day before yesterday I was entertained at Bob Hauser's house in Madison.

VDT: It's nice to see a dynasty within the profession. It was a son of David Brinkley who used the expression "demographic dynasty" first in my hearing about the Taeubers.

RYDER: There were two Taeubers at the affair on Monday night--Karl and Alma.

VDT: Did you work with them?

RYDER: No, I didn't have a great deal to do with them except for helping to hire them. We wanted them and found ways to make it possible for them to come to Wisconsin. The Taeuber that I knew best was Irene, because she was on the OPR staff, though based in Washington, and would come to Princeton with a fund of inside Washington stories. She had a style of communicating them that was almost conspiratorial, as if she was letting us in on something that we should not breathe a word of to anybody else. It was all very exotic for a young kid from Canada to be able to listen to this.

VDT: Did she tell stories about people in high places in Washington?

RYDER: She had a fund of information.

VDT: Perhaps seeping through the walls at the Library of Congress. That's interesting; I haven't heard anyone say that about Irene. She must have been fun to work with, besides being such a top scholar.

RYDER: She was an extraordinarily competent person, but a very warm person. I've never lacked for good friends in the profession. Frank Lorimer would be worth at least an hour on tape all by himself.

VDT: Tell me something about him. He's a person I most regret not having been able to interview.

RYDER: Let me tell you just one little story, because I happened to be on hand and heard it from him. It was after Faith Williams had died; Faith was his wife for many, many years. She had an outstanding career in the government as an economist. Frank had lost Faith and he was, I would think, in his late sixties and we were at the Princeton Inn, which at that time was still a hostelry, it's now a residence, having breakfast on a Sunday. I found it difficult to have a conversation with a man who had just lost his wife of so many years and it was a good marriage. But I said, "What do you think you might do now?" He said, "Well, I've always wanted to go to Africa, so I think I'm going to Africa." And I thought, isn't that great for a person who as far as I was concerned was an old man, not much older than I am now, but that he should just be able to start a new chapter in his life.

Well, while he was in Africa he was in a bar in Nairobi and he picked up a nurse sitting at an adjoining table. She was a nurse from New Zealand, and he married her. It was not long thereafter that Frank came to the Population Association meeting, which was being held at Pennsylvania that year [1963], and I remember our meeting at a big cocktail party in the museum. Frank Lorimer was strutting across the floor like a turkey cock--his wife was pregnant!

I kept in close contact with Frank, as close as I could, because he was so many different people at the same time. His career included being a minister, a psychologist, a research assistant to Fred Osborn, whom he managed to convert away from the rather racist eugenics of Osborn's uncle and really reformed Fred, turned him into an entirely different person. He was also an anthropologist; he was so many different things. He was a very good friend to me, and when I was young I got into the International Population Union [IUSSP] when I was 27 years old [1951].

VDT: You must have been one of the youngest ever to be elected.

RYDER: Yes, I think John Hajnal got in at about 26 or 27. Yes, it was quite young. It wasn't because of extraordinary merit. It was because Frank Lorimer was the General Secretary of the Union [Administrative Director, 1949-57] and he decided that the Union was a little bit narrow and stuffy and they needed a bit of young blood and they needed to enlarge their province. There were very few Canadians. The Dominion Statistician was a member, but that was about it. Had I been an American graduate student, American resident, I wouldn't have stood a chance of getting in for about 15 or 20 years. There were all sorts of eminent people not yet in the Union. I got in because I was a Canadian.

VDT: Oh, come off it! But I got in too, perhaps, as a Canadian. They always put proudly your country of birth. The Canadians are a very strong cell within IUSSP now. I remember you at the Florence meeting in 1985 at that meeting of Canadians when they were talking about wanting to have a national committee again, which was the way the IUSSP was constituted before World War II, which made so much trouble because they became politicized. You got up and gave those Canadians a talking to. That brings me to a question I've been dying to ask. As a Canadian I know how much you are lionized by the Canadian demographic community, which is still small. You went up there for a sabbatical year--about two years ago?

RYDER: Four.

VDT: And they look upon you--you and Nathan Keyfitz--as their proudest products. Prodigal sons that left, but you came back as lions. Tell me something about that.

RYDER: I did intend to fulfill the sacred obligation of being a Canadian demographer, but I found that I loved research more and it was not possible in Canada--nor is it easy to this day--to do research.

VDT: What about the University of Western Ontario? It's getting to be more of a center.

RYDER: The University of Western Ontario is a reasonably well-established population center, as are several others: Montreal has a very strong group, and so on. But I'm not really speaking of the quality of the people so much as the niggardly funds that they have to work with. There are no supporting funds for the center at Western, for example. They don't have any money. If they wanted to buy an annual vital statistics report there would be no funds to tap to do that, for example. They don't have any money to buy a calculator or something like that. The research grants that they can get from Canada Council don't seem to come with overhead on them, which is the staple of American research financing. I think that they are in very hard-pressed shape. Furthermore, since the Mulroney regime

came into power in Canada, a Reagan-like attitude toward the financing of research has prevailed. In particular, the budget of Statistics Canada has been cut unmercifully.

I have been serving the last four years on a demographic advisory council to Ivan Fellegi, the Chief Statistician. We have been attempting to help the development of their demographic and statistical programs under the greatest difficulty because they literally have inadequate staff even to perform the most routine activities, let alone to improve their product. It's been most distressing to me to see this. I have been working this past year--in fact, I spent 15 months--on a small contract with Statistics Canada to develop a new method of forecasting fertility and working closely with Anatole Romaniuc at Ottawa. I hope that will continue to be a lively activity of mine for many years.

I don't know whether you know or not, but I own a piece of Canada. I have a house on Lake Simcoe in Ontario and I spend four months a year there. It's quite handy to Ottawa and Toronto and the like. I would like very much to, in a sense, pay my dues.

VDT: That's good to hear. Some Canadians kind of resent those who went south of the border and didn't come back. Is your wife Canadian?

RYDER: Yes.

VDT: You've had a long and happy marriage, I know.

RYDER: Forty-one years, as of last week.

VDT: Did I see her name listed in the preface to The Demography of Tropical Africa [1968] as being one of the ones who did some of the charts?

RYDER: No, I think you've got the name wrong. I think Westoff's wife . . . and Daphne Notestein was for a long time a chartmaker for Princeton books. She had an important role. If you look, for instance, at the book on The Future Population of Europe and the Soviet Union [1944] and the other books that came out at that time, you will find that those are beautiful charts. Those are Daphne Notestein's charts. Daphne is still alive; she's 91 years old. She only just this past winter stopped driving her car, not on the grounds that she was incapable of driving a car, but if anything should go wrong, they would blame it on her age and she didn't want them to have the opportunity.

VDT: About PAA, you've already intimated how PAA has changed over the years. You have said that when it was small, everybody knew everybody. The changes now--what do you think of having 87 overlapping sessions, as we just did in New Orleans?

RYDER: I want very much not to sound like an old fogy, but I regret very much the loss of what we once had. I do not find a large hotel to be an intellectually exciting environment. Particularly a large hotel like the one in New Orleans which didn't provide anybody a place to sit and talk with anybody else. The multiplicity of sessions, I suppose, is an acceptable facade for assuring that lots of people get their expenses paid.

VDT: Get their papers accepted, so their expenses are paid.

RYDER: Yes. But, maybe it's a personal failing. If a paper is worthwhile I want to be able to study it, meaning to read and reread. I can't pick up a decent paper through the ear at all. I don't find the meetings to be satisfactory for paper presentation. If a paper's good I will read it, but I won't go and listen to it.

It is possible to have meetings, I think, at which people who are known to be opposed on particular salient issues are actually engaged in confrontation. And there have been occasional meetings when, sometimes by design but mostly by accident, it happened to fly. It's very much like live TV as against canned TV. It would be nice if some of our meetings could be a little more structured toward nailing down issues on which people are apart and ought not to be.

VDT: Some interviewees in this series have said that in the past you could hear Kingsley Davis, with his drawl, out after Frank Lorimer, always confronting each other. They have tried to do that in recent years--this year's [1988] "Authors meet critics," the Michael Teitelbaum session.

RYDER: Yes. Another problem, of course, is that as the profession has increased in size, it has also increased in specialization. It's the nature of our modern bureaucratic world. That means that many sessions are quite uninteresting to many other people. Furthermore, the only way you can make a contribution now is to chisel away at one little corner of the enterprise and the refinements are rather boring in oral form.

VDT: Well, maybe you have yourself to blame. You said you set up the questions and now you have some of your followers, students, chipping away at the corners. I had a question on what you see as the outlook for demography, and you have perhaps answered that. Is there still room for the great overarching theory questions, which you said you have attempted to set up during your career? And you've paved the way for people to chip away at the little questions. Is it too late for young demographers other than to chip away at the smaller questions?

RYDER: I have not gotten that sense yet of closure in any sense. There are many large stumbling blocks that we have in front of us and it's hard for me to see how they might be resolved. I think that we have pretty well gone to the limit with straightforward survey interview techniques, for example. I don't think we are going to make a substantial advance merely by refining questions. I think we face the incredible difficulty that if the subjects are genuinely important to people about whom we are inquiring, we can expect dissimulation and non-response and lying and bending the truth and so forth, just because it's important. And of course, as you know, non-cooperation has been rising throughout the whole survey world. I don't see much of a future in a host of bigger and better surveys.

VDT: You're not suggesting that the National Survey of Family Growth should be discontinued; it should go on collecting the data?

RYDER: Oh, no. We need avenues for collecting basic behavioral data. I think questions attempting to get at why people do what they do are essentially beside the point, that's my feeling. It's a good idea for us to monitor this very important kind of behavior and do it as well as we can. But the stuff of explanation, I think, will not lie in that direction, as far as I can see.

There is a core of demography, formal demography, in which there are rather few questions, partly because at the core, demography only has a couple of crucial variables and parameters, and probably there's not a living to be made by a large number of people. But I'm quite enthusiastic about the prospect for, in effect, doing experimental, almost mathematical, inquiries using computer simulations. I think that's a large area for imaginative endeavor. There are some excellent young people, much better trained than I ever was, who are showing good signs in that direction.

But I think we have perhaps arrived at a point in demography where we have to get serious about questions of the understanding of social behavior and turn ourselves much more explicitly into social scientists. I think the easy returns from a demographic training are ones that are pretty close to being exhausted. We've now got to get on with much more difficult questions that will not yield

easily. They are questions we have in common with any student of social change. The whole theory of social change is in chaos and the demographic transition is a piece of that chaotic situation.

VDT: That's a good point to end on. It leaves you plenty to do for the future when you are, quote/unquote, retired. Will you spend more time in Canada, more than four months, although there are barely four months of bearable weather?

RYDER: If the weather's good, we'll be there.

CONTINUED

VDT: We've picked up again. Something about Norman that I had not known is he's a musician, a piano player all his life. Explain that.

RYDER: Let me explain how I am anywhere near the category of musician. When I was a child, I learned the piano as so many children do, but became completely captivated by the instrument. I was from a working-class family and we couldn't afford more than a year's lessons. So from then on, I used to go to the music store and I would borrow a book which represented the next year's work in music and come home and copy it out and take the book back and say that my mother wouldn't give me the money for it.

VDT: You literally copied the music?

RYDER: I invented a little system of musical shorthand that I could get the thing done rather efficiently. When I was 14, I took up lessons again, again very briefly--another six months--and then had to have a serious conference with my teacher and with my father and mother, because either I was going to have to spend the money on training for the piano or I was going to be able to go to college. The question was which; it couldn't be both. Times were hard then, even for very good piano players, because it was the Depression. And the decision was made that I should go to college instead.

Now, I played the piano professionally in the sense that I belonged to a small jazz group. I remember that we played week after week for a dance at a small private club and in return they would give us practice space during the day. It was a good deal for us and gave us a lot of good practice. This was in Hamilton; I guess I was 13 at the time or 14. One day my mother said, "You are not going to go and play at the club tonight." I phoned my colleagues in the band and their parents had said the same thing to them. I don't know what their source of information was, but the club was raided that night. There happened to be a bordello on the second floor, which we had no awareness of.

Just to wind this up, when I came to the United States, in order to pay moving costs I had to sell my piano. And it was many, many years before I got enough money saved to buy another one. So my piano sort of lapsed. However, I was at a party of the sociology department at Wisconsin and got to playing the piano again and one of my colleagues, a professor named Hans Gerth from Germany, came over and said in his very German accent, "Ryder, you play a very good whorehouse piano." He had me nailed!

VDT: Had you had lessons?

RYDER: As I said, I had a year's lessons when I was eight and six months when I was 14. The piano was very much part of my life when I was a Canadian and when I came to the States I had to redefine myself, because I was always known as the piano-player up there. Down in the States, most people have no idea that I ever played the piano.

VDT: I never did. You play now regularly?

RYDER: No, I don't play very much. It's one of the things I will certainly do a lot of when I get a little free time.

VDT: I hope you will. And you say that Dudley Duncan has taken up much more composing?

RYDER: Yes. This has been an interest of his for a very long time, and he now has the feeling that as an emeritus he's entitled to do whatever interests him, and this interests him very much.

VDT: That's what an emeritus should do. Thank you.

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Norman B. Ryder
1923-2010

The distinguished sociologist and demographer Norman Ryder died in Princeton on June 30, 2010, at the age of 86. Ryder was born in Hamilton, Ontario, on August 24, 1923. After receiving his BA from McMaster University and serving two years in the Royal Canadian Navy, he earned his PhD.from Princeton in 1951. He then served on the faculties of the University of Toronto, Miami University of Ohio, and the University of Wisconsin before returning to Princeton in 1971 as professor of sociology. He retired as professor emeritus in 1989.

Ryder's signal contribution to social science was his elaboration of the concept of the cohort as a mechanism of social and demographic change. A cohort is a group of people who enter a population during the same period of time and go through life together experiencing common circumstances and events. The concept of the cohort proved to be pivotal in allowing demographers to understand the interplay between the level and timing of fertility and how they interacted to determine the rate of childbearing at any point in time. In presenting Ryder with the Laureate Award on behalf of the International Union for the Scientific Study of Population, the distinguished French demographer Jacques Vallin noted: "you are the father of a method that no serious demographic textbook can afford to overlook."

The concept was also influential in helping sociologists understand the process of social change. Ryder demonstrated that changes in a population's attitudes and behaviors over time occur as much through the dying out of older cohorts with old ideas and the coming of age of new cohorts with new ideas as from living people actually changing their minds. This realization did not stop him from trying to persuade colleagues of the virtues of cohort analysis. As he noted in a 1988 interview, "I've spent my whole professional lifetime as a salesman for the cohort approach."

Ryder is known for a series of landmark studies of changes in the reproductive behavior of American women that he launched during the 1960s and 1970s with his Princeton colleague Professor Charles Westoff. Three rounds of the National Fertility Studies were conducted in 1965, 1970, and 1975, with thousands of interviews with women on issues such as contraceptive use, unwanted births, sexual behavior, fertility expectations, and childbearing behavior. Before this time it was not widely accepted that survey researchers could ask women intimate questions about sexuality.

Results from these studies were presented in two influential and widely cited books, *Reproduction in the United States, 1965* (1971) and *The Contraceptive Revolution* (1977), both published by Princeton University Press. Together they revolutionized demographic thinking on human fertility, provided data to untangle the period-cohort dynamics of the baby boom, and helped document the sexual revolution of the 1960s, thereby gaining notoriety both within and outside the academy.

While continuing to publish widely on fertility issues, including more than 100 journal articles and book chapters, Ryder also advised policymakers in the United States and in his native Canada on many aspects of population studies throughout his career. His many contributions were recognized by peers and colleagues inside and outside the field of demography. He was elected to the Sociological Research Association in 1967 and served as its President in 1974-75. In addition to being named the 2000 Laureate of the International Union for the Scientific Study of Population, Ryder was elected as a fellow of the American Academy of Arts and Sciences and the American Association for the Advancement of Science, and he received the Population Association of America's Irene B. Taueber Award for outstanding achievement in demographic research. He was also awarded honorary doctorates by the Vrije Universiteit Brussel and the University of Montreal and named an Outstanding Alumnus of McMaster College.

Norman Ryder was an inspiration and mentor to generations of students both at Princeton and at other leading demographic training centers around the world. He was also a devoted husband to his wife of 63 years, Helen, his son Paul and his daughter Anne, and was beloved by four grandchildren and one great grandchild as well as his students and colleagues. He will be greatly missed at Princeton and by everyone in the fields of demography and sociology.

Douglas S. Massey, Thomas J. Espenshade, James Trussell, and Charles Westoff, Princeton University

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A CRITIQUE OF THE NATIONAL FERTILITY STUDY*

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A substantial part of our knowledge of American fertility is based on the results of a series of national surveys. In this paper, I propose to examine some characteristics of those surveys which depreciate the value of their output, and suggest, in passing, some alternative procedures.

Although the title of this paper refers to the label attached to the surveys conducted in 1965 and 1970 by Charles Westoff and myself (Ryder and Westoff, 1971), its scope encompasses as well the Growth of American Families Studies of 1955 and 1960 (Freedman et al., 1959; Whelpton et al., 1966), associated with the names of Pat Whelpton, and Ronald Freedman and Arthur Campbell, and its implications extend to the survey by the National Center for Health Statistics, under the leadership of John Patterson and William Pratt, planned for the summer of 1973 (God and OMB willing), and the various national pieces of the World Fertility Survey, on the horizon. Indeed, most of the criticisms are directed not so much at the way in which the task has been executed as at problems which are intrinsic to the cross-sectional survey as a statistical species or implicit in the complexities of the phenomenon under investigation.

There is, admittedly, something untoward if not perverse in undertaking to criticize my own work and that of some close associates, but I have been impelled to do so by the opinion that the results to date have been too readily accepted. Perhaps that is because we of the demographic profession are closely knit, like a kind of extended family, and tend to be gentle in our comments about each other's work, at least in print. On the other hand, we may have been so successful in hedging our output about with methodological thickets that nobody who's outside can perceive what it looks like from inside. Whatever the reason, we have been let off lightly so far. Yet this is no exercise in self-denigration nor an act of betrayal of colleagues. I view criticism as a form of compliment to the researcher, implying that the work in question has sufficient intellectual importance to deserve close scrutiny rather than benign neglect. In the oral version of this paper, my intent was to be provocative, and so my derogatory remarks were unhampered by efforts to achieve judicious balance and perspective; in what follows, there is some attempt at redress of grievances.

Every fertility survey worthy of the name has the following facets: (1) a definition of the universe in light of the purposes to be served by the study; (2) a set of measurement procedures oriented to the dependent variable; (3) inquiry into the intervening variables of fecundability and efficacy; (4) the probing of reproductive intentions; and (5) an attempt at explanation of the findings. My overall charge is that our studies have been flawed with respect to every one of these components, and I propose to cast a jaundiced eye on each in turn.

* Presented as a Presidential address to the Population Association of America at its annual meeting in New Orleans, Louisiana, April, 1973.

Definition of the Universe

Our problems begin with a surfeit of purposes. We are expected to use our left hand to describe and explain while we use our right hand to predict and give policy advice. To illustrate how this diffuses effort, consider that, if one wants to forecast, one focuses on young women, whose babies are still to come; if one wants to evaluate family planning programs, one must check on women in midstream, who have had their quota and are ready to stop; while if one is primarily interested in understanding what has happened, one's attention should be fixed on women who have come to the end of the line.

The catholicity of scope is also revealed in the ways we must cope with different audiences. Some of our output must be cast in the form of national totals to provide appropriate ammunition for journalists and politicians, some of it takes the fancy biometric form of life tables of fecundability and contraceptive efficacy; and some of it is cloaked in the language of the testing of sociological hypotheses.

Perhaps we have been tempted into trying to serve so many masters at once because of our memories of the poverty-stricken infancy of social research; we act as if each study were the last and only chance to fulfill all objectives. One regrettable byproduct of our excessive ambition is that it takes a very long time to finish each study and get the book out; our record is almost as blemished in this regard as the national vital statistics. Now time considerations should not bulk large in the minds of those concerned solely with adding to the sum total of human knowledge, sub specie aeternitatis and all that, but they surely matter for data which are also designed to serve those responsible for practical problems and programs.

The problems of multiplicity of purpose pervade the realm of social analysis as well. We investigate too many areas and, in consequence, fall short of sufficient depth in each: we have some biometric data but not enough to come to grips with the complexity of reproductive behavior; we have some attitudinal questions but pursue our inquiries so briefly on any one topic that a social psychologist must be left with a feeling of continual frustration; our forays into the material facets of family life are unsatisfying to the economist; and so on.

I suppose the problem is that, even if social research in general were not regarded with mistrust, the considerable expense of this kind of inquiry might prohibit its funding if we did not offer a package with some attractiveness for each of many interests. Our diffuse and shallow orientation, and our mixture of pure and applied objectives, are the price we pay for the opportunity to do the studies at all. I trust that our discipline, and those who finance its research, will soon mature to the point of designing studies each of which has its own particular focused thrust, uncompromised by conflicting objectives, and abandon the cross-sectional cafeteria.

The conventional definition of the universe for the multipurpose survey is the total population of females who are currently married and still younger than 45. A problematic aspect lurks behind each facet of this definition.

On closer inspection, the target turns out to be, not the total population, but rather that of the conterminous United States able to respond to an English-language questionnaire and captured by a household sample. For some purposes, the last of these may be nontrivial; young adults in particular often live outside the bounds of the conventional household. More important is the circumstance that we cannot collect data from those who cannot be found nor from those who are found but are unwilling to talk. The contrasting coverage of the four surveys of 1955, 1960, 1965 and 1970 with respect to blacks is in-

structive. They were excluded from the design of the first survey; their numbers were too few to permit more than zero-order tabulations in the second survey; and, although the sheer problem of size of subsample was overcome in the last two surveys, it is evident from a comparison of the characteristics of black respondents in 1965 and in 1970 that they represent two quite different selections. In particular, it seems that interviewer timidity and respondent resistance has resulted in a 1970 sample which is deficient in representatives from the urban ghettoes. But at least we know this to be the case and are taking appropriate precautions to avoid inferences which confound procedural and behavioral differences.

The next distinguishing characteristic of the universe definition is its focus on females. While the reasons of convenience and economy behind this decision are understandable, the goal of the inquiry is in fact the analysis of fertility, and that does encompass male as well as female exposure to risk, male as well as female fecundity and fertility regulation, and male as well as (and combined with) female reproductive intentions. It would seem that we regard men as so insignificant in the reproductive scheme of things that all we need to know about them is hearsay testimony. Yet the decision is not mere oversight. The marginal revenue of male data, in the sense of proportion of variance explained, in the Princeton Fertility Study was far less than the marginal cost of obtaining those data.

The marital status criterion raises several questions. To be eligible for interview, the respondent must be married with husband either present or absent for reasons other than marital discord, to use the felicitous census phrase. The point of this is, of course, that only such women can be asked, without much embarrassment, questions about their current contraceptive use and reproductive intentions. But such a choice has unfortunate implications with respect to noncurrent data, i.e., the wealth of information about the respondent's reproductive history. Even if all those exposed to risk of fertility were married, and all those married were exposed to risk, the women who are currently married, as of interview, are those who happen to be caught inside rather than outside marriage at the time. Age by age their constituency is transformed by marital dissolution and remarriage. Complete histories of married life can only be obtained by studying the ever-married; this is an innovation of the 1970 Study. Obviously, any comparisons with the previous studies still require restriction of the tabulations to those currently married, even though the population so defined has a nebulous aspect to it.

The extension of coverage to the ever-married is ineffective in coping with two major flaws. First, in order to explain fertility, one is obliged to explain nuptiality as well. Exclusion of the never-married breaks the cardinal survey rule that no sample criterion should itself be part of the dependent variable. Second, the appropriate distinction for a fertility inquiry is, in any event, not between the ever-married and the never-married; rather it should be between nonvirgin and virgin. We have been reluctant to grasp this nettle and continue to use the married state as a delicate euphemism. Now it is true that respondents often confess about premarital conceptions—from which we cleverly infer that there has been premarital copulation—but such data are demographically useless unless we know also how many have been exposed to risk without getting pregnant. One consequence of this difficulty is that we have abandoned in the 1970 Study any attempt to measure contraceptive efficacy prior to the first conception. Yet it would be too facile to say that there is a right

way and a wrong way of proceeding with respect to the collection of information about the exposure of the never-married. The basis for the decision is the comparison of the gains from acquiring data about those willing to volunteer the information required (undoubtedly a select subgroup) and the losses from alienation of affections. The work of Zelnik and Kantner (1972) has clearly reduced our responsibility in this regard.

With respect to the age criteria, our four national surveys have, in fact, four different specifications. In the first two, the criteria were in terms of age in completed years at interview of 18–39 and 18–44, respectively; in the last two, the criteria were in terms of birthdates no earlier than July 1, 1910, and July 1, 1925, respectively. Strictly speaking, then, birth cohort, rather than age criteria, were employed in 1965 and 1970, and consequently age at interview is a function of interview date (which averaged November 10, 1965, for the former and December 31, 1970, for the latter). Accordingly, in the published results of the 1965 and 1970 Studies, those identified as being a particular age are in fact a little older than that, and more so for 1970 than for 1965.

There are two notions behind the introduction of an upper age limit: to waste no time on those who are too old to have any more than an occasional baby; and to avoid the gross departures from reality that may occur when one asks a respondent about what she did long ago. Recognizing the general validity of those points, the question remains why age is chosen as the appropriate life-cycle location for analysis. If respondents have to have been married, then the obvious variable to use in locating them with respect to personal or historical time is marital duration. Since age at marriage does vary from group to group, and over time, the life-cycle observations in these studies, almost all of them presented age by age, are a confused mixture of age at marriage and marital duration. This is another example of the difficulties associated with a multiplicity of purposes. The 1955 Study, which established the precedent, was framed in age terms because the birth statistics for the United States come in that form and the survey was perceived in part as a way of extending the (birth) cohort fertility tables into the future (Whelpton, 1954).

This should not be taken to imply that marital duration itself is a completely satisfactory choice as the variable of temporal location for the purposes of life-cycle description and trend determination. Marital duration has been defined in all four studies as the total lapse of time between date of first marriage and date of interview and, accordingly, conveys a quite different meaning for those whose first marriage was dissolved than for those with one unbroken marriage. Nevertheless, in hindsight, my own preference for a study design would be one keyed to the criterion of year of first marriage, ignoring those whose experience is too brief for responsible assessment and those whose experience is too distant to be well remembered. For comprehensive fertility coverage, this would necessitate supplementary studies of dissolution and remarriage, on the one hand, and an examination of birth cohorts of never-married women, on the other.

Measurement of the Dependent Variable

The circumstance that the universe is defined with respect to both age and marital status as criteria causes major difficulties for the calculation of demographically defensible indices of (approximately) complete fertility, whether the purpose is some kind of time series or the cross-section of sociocultural or other differentials. The respondents range from quite young to quite old in a reproductive sense, and the amount of experience they have to talk about tends

to vary directly with age. Were one to collect data for a series of cohorts or a series of periods covering the entire age span, one would end up with age-time rectangles of data to examine, whereas the retrospective histories from a cross-section by age yield only a triangle of data. If one were to consider the events within that triangle indiscriminately with respect to cohort identification or time of occurrence, one would produce well-nigh meaningless measurements for analytic purposes, and yet these are the kinds of numbers for which there is the biggest demand. For example, how many women have ever used the pill, or how many have had an unwanted birth? To the demographically pure in heart, such questions are nonsense.

There are several ways of deriving indices of complete experience from this triangle of data, none of them satisfactory. Consider first a cohort approach. We can report the behavior of our oldest group of respondents (those who are 40–44 plus about one-half a year as noted above). This is less than adequate, partly because it leaves a lot of evidence (for the younger respondents) unutilized and it is not even quite complete in a reproductive life-cycle sense, but particularly because most of the events summarized occurred several decades ago (and what may have happened then may be ill-remembered, or deftly reconstructed). Since the maximum interest in the study is generated by its description of contemporary experience and portents for the future, the result is unlikely to make headlines. Furthermore, despite my penchant for viewing the world from cohort eyes, I must admit that our recent demographic past has been so distinctively marked by the period-specific stimulus of the introduction of modern contraceptives that the pristine elegance of the cohort approach must bow before the weight of stubborn facts. The final difficulty is that, since this is an ever-married birth cohort, whatever differentials are observed are the combined outcome of nuptiality variations as well as marital fertility variations. And that difficulty cannot be resolved by shifting to a marriage-cohort approach because the earliest marriage cohorts in the study (and thus those with approximately complete histories) are necessarily, because of the upper age limit on our respondents, restricted to moderately young ages at first marriage.

One can avoid the bias in age at first marriage and achieve results for a more recent marriage cohort to boot, if one is willing to resort to what may be called the expectations gambit; this involves taking the respondent's actual past fertility and adding to it her guess as to her future fertility. This does give us some kind of number for every respondent, but the outcome is a bastard mixture of hard and soft data, with the proportion of each depending on the respondent's reproductive location. It would not be much of a parody to claim that the typical respondent recounts to us a catalogue of her past follies and then swears that her future will be free of error. But mistakes do happen, as well as changes of mind, and they happen differentially. I find it hard to know what to make of the expectations concoction.

The final approach is to have recourse to the synthetic-cohort trick—cumulating the experience of women at the various life-cycle stages, duration by duration, over, say, the most recent five years. This does give a contemporary result, but its potentialities for distortion of reality should be well understood by anyone who has watched the gyrations of the period total fertility rate in the United States. And no amount of detail concerning specificity by parity or birth interval or whatever can purge the measure of that distortion, since it originates in the disposition of successive cohorts

to adopt different time patterns of fertility.

Our resolution of this array of difficulties, in the study of fertility differentials for the 1970 Study, is to present indices for both the earliest real birth cohort and the latest synthetic marriage cohort. As the foregoing account indicates, neither is completely satisfactory, but we hope that the combination will protect us from egregious errors of inference, and we have the small satisfaction that all of the alternatives are worse.

Turning now to the question of constructing time series from our data, one would expect from the existence of four surveys at quinquennial intervals that we would be in a strong position. The sad fact is that such a time series analysis requires survey comparability, and we have been unable to resist the temptation to change the ways we do things. Each new survey has had somewhat different sample criteria and somewhat different receptions by the public. Three different survey organizations have been used, and the questionnaires have continued to evolve in ways which may represent progress in methodological sophistication but play havoc with time series requirements. It is impossible to say with any assurance, for example, whether unwanted fertility went down a little or a lot in the interval between the 1965 and the 1970 Studies. We are playing "on a cloth untrue, with twisted cue, and elliptical billiard balls."

Now obviously one should make amends whenever one finds that previous procedures have led to error, but the cognate responsibility is to find out how much difference in the results a procedural change produces. We have not lived up to that responsibility. In defense, however, we point to the rapid pace of methodological advance, which suggests that standardization of measurement procedures would be premature and the splitting of the sample between those subjected to the old and those subjected to the new questionnaire would be costly. We have the strong impression, moreover, that the refinement of our measurement procedures considerably exceeds that of our analytical posture.

Even with comparable surveys, some way must be found to cope with the aforementioned tendency, induced by our design, for age at marriage to vary directly with year of birth and inversely with year of marriage. In our Population Commission paper on wanted and unwanted fertility (Ryder and Westoff, 1972), we devised a way out of this dilemma by blending early birth cohorts with late marriage cohorts; we think the trick worked, but it's almost impossible to explain to anyone.

Exposure, Fecundability and Efficacy

A fertility survey offers the exciting possibility of digging down underneath the sequence of births to reveal the intermediate variables of exposure, fecundability and efficacy. First, we try to divide the respondent's lifetime into periods of copulation and of noncopulation. Ordinarily we pretend that this is provided us by the information that a respondent is or is not currently married at any particular time, although we do pry into extended stretches in which the currently married are without a bedmate.

Then we attempt to subdivide the periods of exposure into fecundable and nonfecundable segments, for example by eliminating the months a woman cannot get pregnant because she is already pregnant. The trouble here is that many respondents misstate their numbers of pregnancies. Our evidence on this score is that fewer than two percent of our 1970 sample said that they had ever had an abortion, whereas our best guess is that the true figure exceeds that by a factor of more than ten. Moreover, the counts of miscarriage are likewise

suspiciously low. In consequence, we are unable to make the due amount of subtraction from fecundable segments for time pregnant prior to these unreported events, and we end up with inflated estimates of contraceptive efficacy and deflated estimates of fecundability.

The final step in the procedure for establishing values of the intermediate variables is to divide the fecundable segments into those which do and those which do not involve use. To be precise, we ask about the fact of use within a segment but not about the duration (or regularity) of that use. Documentation of the contraceptive revolution is probably the most publicized success of these studies, but its underpinnings are shaky. Simply speaking, the data show not so much what happened as what respondents choose to say happened. It is likely that many responses have been tipped in the direction of whatever the respondent regards as a more favorable response, and especially if there is at least plausibility to the misstatement. A long interval would make suspect a report of nonuse, but not a report of successful use (even if actually a failure). Moreover, some actual users feel guilty about using or feel ashamed about using and failing and avoid either stigma by suppressing the fact of use altogether.

The crux of the problem here is that we conceptualize the process as a temporal sequence which begins with the intent, is followed by use or nonuse to correspond with that intent, and ends with the outcome. Nevertheless, all of these data are obtained at the same time, and often long after the experience, and it is not unlikely that respondents tailor their statements of intent and use to fit the outcome that they well know. If we could confidently assume that such errors are approximately constant over time and much the same for different subsamples, we would be tolerant of such frailty, but that assumption seems untenable. Indeed, we are forced to contemplate the possibility that the trends in behavior that we observe are in fact trends in the willingness to report that behavior.

Our ability to distinguish between use and nonuse segments and therefore to be able to make separate measures of contraceptive efficacy and fecundability, respectively, is dependent on the existence of an unequivocal distinction between the two types of behavior. Yet many actions which affect the probability of conception may or may not be contraceptive in intent. We have in mind the immortal "douche for cleanliness only" and its modern counterpart, "pill for medical reasons only," as well as lactation, sterilization, and even abstinence and other perversions. The distinction between use and nonuse in all such cases depends not on the recall of something that happened, but on the recall of the respondent's state of mind when it happened. And there is a further conundrum, even with completely accurate and honest reporting. Couples who are more fecund are selected toward use, while those who are less fecund are selected toward nonuse. A good measure of contraceptive efficacy requires a comparison between what does happen and what otherwise would happen, but how can one get a reading on the fecundity of users? Certainly they are unlikely to cooperate by making a test on our behalf, no matter how deeply they care about scientific progress.

The final obstacle in the way of making worthwhile estimates of levels of fecundability and efficacy is the complexity of the measurement problem. The proper procedure involves a life-table approach in order to merge closed segments (those which end in conception) with open segments (those which do not). Furthermore, the triangular arrangement of data, referred to above, carries the implication that there will be a larger proportion of segments of any particular kind which begin at an

early age than would be the case for a cohort reporting its completed history. Since we know that the age at which a person experiences an event, such as the occurrence of a birth of a particular order, strongly influences subsequent behavior, failure to introduce a control for that "initial age" would lead to biased results. A few years ago we published, in *Demography* (Ryder and Westoff, 1969), some estimates of the proportions of women with at least one unwanted birth which failed to take that circumstance into account; it is now apparent that those results were biased upward for that reason (even though they were probably biased downward for other reasons). There is a small consolation, however, in the realization that almost every analysis of cross-sectional data, whatever the topic, has perpetrated this kind of error.

The upshot of this argument is that our sense of demographic responsibility demands that we calculate a host of life tables—several thousand were produced in my latest tabulation request—in order to achieve estimates of contraceptive efficacy and fecundability. Such an inundation with detail inhibits us immeasurably in attempting to answer what seems like such a simple question: How effective are American couples in their use of contraception? It is worth noting, however, to provide perspective on our difficulties, that there is likewise no demographically responsible answer to the question of what the current level of fertility is in the United States, and for the same kind of reason.

Reproductive Intentions

Close to the heart of the Studies of 1965 and 1970 has been the challenge of classifying planning failures as accidents of timing or as unwanted births (accidents of termination). Since our Population Commission paper on this subject has provided a detailed account of our difficulties, a few comments here will suffice. Our procedure once we had ascertained that there was a pregnancy which was not wanted at the time it occurred was to ask the respondent whether, just before she became pregnant that time, she wanted a baby later or never. Although this approach caused no difficulty in eliciting responses from most of our women, and the structure of answers seems intelligible, there are some problematic cases. For example, there may be no answer to the question. From the standpoint of the actions necessary to fulfill their reproductive intentions, all that a couple needs to have in mind is whether to permit the next ovulation to come to fruition. Should they decide in the negative, use contraception, and fail, then they know that they have failed to prevent the pregnancy, but they may not have had any opinion before the fact as to whether they were trying to delay or to terminate. For others, the answer may be difficult because their minds were less than certain on the subject. Since intentions presumably depend on the developments of a cloudy future, we should expect them to change over time for any particular couple, and perhaps with much vacillation. This circumstance poses a difficult technical problem. The proper way to handle any cross-sectional survey data on the frequency of occurrence of events is a life-table procedure; that involves, as noted above, the merger of closed intervals (for those to whom the event has occurred) with open intervals (for those to whom it has not). For the former, we ask their state of mind prior to the event in order to classify that event properly as a timing failure or a termination failure, but for the latter, our only information is their state of mind as of interview, and that may have been quite different had we interviewed them somewhat earlier in what is often a long open interval.

The wicket is even stickier if we relax the assumption that the respondent tells the truth and admit the possibility that she may choose to tell us not how things

really looked at the time, but rather how things worked out eventually (or how she would like us to think they worked out). Everyone is familiar with the old saw that causation cannot be inferred from correlation. We run the further risk of providing the respondent with the opportunity of reconstructing her own cause-and-effect sequence. Conceivably we are measuring not so much the incidence of unwanted births as the extent to which the couple can summon up the resources necessary to cope with and rationalize such occurrences.

In the last analysis, what conclusions are derived from our research for the purposes of analysis and policy are less a matter of hard data than of judgment. We think our judgments are pretty sound, but we shouldn't be surprised if there are some who disagree. Such qualitative appraisals, supplementary to the statistics, are commonplace throughout social science, as is dissensus, and that is probably a healthy state of affairs.

There is an ironic twist to the policy debate that has swirled around the topic at hand. On the basis of 1965 data, we asserted that the excess of fertility above replacement was mainly if not wholly a matter of unwanted births, and concluded that what would bring down the birth rate would be more use of better contraceptives. There is, indeed, more use of better contraceptives now, and the birth rate is, indeed, down. Q.E.D. Nevertheless, our 1970 data suggest that the decline in fertility over the preceding five years was due as much to a decrease in the number of children wanted —the prescription of those who disbelieve in the effectiveness of a family planning approach—as to a decrease in the number of unwanted children (Ryder and Westoff, 1972).

Explanation of the Findings

This discussion must begin with the recognition that we demographers have never been big on theory—indeed we almost pride ourselves on that. With specific reference to fertility surveys, the chastening experiences of attempts to test hypotheses in the Indianapolis Study (Whelpton and Kiser, 1946, 1950, 1952, 1954, and 1958) and in its lineal descendant, the Princeton Fertility Study (Westoff et al., 1961; Westoff et al., 1963; Bumpass and Westoff, 1970), have induced us thenceforth to keep our hypotheses in low profile. This can be seen in the style of analysis itself. We eschew the variety of sophisticated multivariate techniques in favor of pedestrian cross-tabulations (and continue to add variables until cell sizes begin to fall below 20, surely the reductio ad absurdum of sampling theory). The most charitable view of this approach is that we conceive our primary role as the establishment of descriptive parameters which will be the raw materials for launching particular studies in depth. Furthermore, one loses one's zest for complex chains of reasoning when confronted with the results of our 1965 Study of inconsistency of reporting, in which the typical behavior item registered about 25 percent random responses and the typical attitudinal item about 50 percent random responses.

And yet we do have a simple implicit model. The basic idea is that couples pick out a reproductive target and then are more or less successful at hitting it. The target chosen reflects the reproductive norms they have internalized; their capacity to hit that target reflects whatever regulatory norms may inhibit them—as well as their general capacity to solve problems, i.e., their education.

This model provokes a series of questions. In the first place, we may have oversold the idea that people actually reproduce according to the means/end schema. We frame our questions as if they do, and our respondents are typically very obliging in falling in with our way of approaching this and any other subject, but that does not deny the possibility that many of them approach the demographic dilemmas of

their lives with quite another orientation. Having registered that doubt, I would argue that attempts at nonrational explanation generally border on the irrefutable and therefore scientifically unacceptable, whereas our unimaginative orientation generates data which have high face validity and pose few mysteries for residual inquiry.

More difficult is the problem of the optimal method for identifying the reproductive targets themselves. Typically we ask: "What do you think is the ideal size of a family?" or "If you could start your life over again, and have just the number of children you wanted, how many would that be?" or "How many children do you really want?" None of these is satisfactory, primarily because we are asking the respondent to perform a complex conceptual experiment: "If everything else in your life were to remain the same, except for your parity, what would you choose for your parity?" I suspect that respondents, faced with this challenge, can scarcely avoid thinking of other things they would like to change in addition to the number of children, such as their health, or their housing, or perhaps their husband, unless, of course, they reject the game altogether and converge on their actual experience. Furthermore, the notion of an ideal or desired family size is clearly a sociological concept, whereas our surveys have been committed to the biology of the situation. What is meant, simply, is that people are interested in children, rather than in live births; men and women frequently have more than one family of procreation, seriatim, and we shouldn't forget Granny in the back bedroom. As sociologists we ought to recognize our vested interests in social arrangements, the proper referent of norms, rather than mere biological output.

More fundamentally, there is no prima facie case that people actually have any reproductive target at all, in the sense of a number they intend to end up with. They may play it by ear, over time, asking the reasonable question: "Do I want to get pregnant this month?" Now, of course, they end up with some number or other, but that may merely represent the arithmetic of a long series of ad hoc decisions. Moreover, a plausible case can be made for the proposition that reproductive decisions are as much oriented to considerations of the passage of personal and historical time as to any cumulative frequency. We have 56 years of birth-rate evidence for the United States that ascribes most fertility changes directly or indirectly to modifications in the timing of reproductive output. Although the four fertility surveys have taken passing shots at the subject, the vast preponderance of attention has been devoted to final parity, and that may be a misplaced emphasis.

We are also, in my judgment, unsuccessful in framing questions which tap the norms governing the various kinds of fertility regulation. To ask respondents how they feel about one or another mode of regulation, the customary procedure, is inadequate, because it leaves us uncertain as to whether they are talking about what they would do (and under what mitigating circumstances they might do otherwise), or whether they are telling us how they would feel if somebody else were to engage in that particular practice (again with the proviso that it may depend on the situation), or finally whether they are placing themselves on some scale of tolerance or of respect for the individual autonomy of others ("It's up to them"). Nor do we ordinarily ascertain the strength of approval or disapproval: What sanctions would the act elicit? Mind you, we share our ineptitude in these respects with researchers in all other areas of social inquiry.

Our studies have yielded abundant evidence that the major explanatory variables are those which identify the membership of the individual in sub-

cultures, social classes and other groups with distinctive life styles. This finding raises doubts about the wisdom of the strategy of surveying individuals. To state the argument succinctly: Reproductive behavior reflects the influence of norms within various contexts. Norms are properties of groups. Therefore the understanding of reproductive behavior requires the study of groups and their characteristics (including norms), rather than the study of individuals and their characteristics. There is a place for the survey of individuals in this analytic schema: first, to ascertain group membership; and second, to examine the process of socialization by which individuals acquire allegiance to groups and the norms which define those groups. Our studies are quite deficient in information about socialization for parenthood, either in the family of orientation or in the family of procreation. We focus on reproducers and their births, rather than parents and their children.

Our final explanatory embarrassment is that, over the time span of these four national surveys, whatever the cross-sectional differentials, the level of fertility has gone up for every subgroup, and then it has come down for every subgroup, and we are far from an explanation of why that happened and whether it will happen again. We have made a contribution to the question by distinguishing crudely between that part of the change attributable to modification of reproductive ends and that part attributable to modification of the means for achieving those ends, but the explanation of those modifications, especially the former, still lie well beyond our reach.

In conclusion, it seems to me that we are close to exhausting the potentialities of the cross-sectional survey as a procedure for enlarging our knowledge of reproductive behavior, beyond the limited albeit useful function of providing periodic benchmark parameters. In my judgment, it is entirely appropriate at this juncture for the government to assume that function. The frontiers of inquiry, which is where academic demographers belong, are now elsewhere. Three directions of inquiry deserve priority: (1) establishment of the correct sequences of intentions, actions and outcomes in people's lives and causal chains in general, by interviewing the same respondents a series of times with appropriate safeguards to permit the assessment of the magnitude of contamination; (2) development of the methodology for measurement of group properties which are presumably interdependent with reproductive norms to set the stage for what would, in short, be a sociology of reproduction; (3) abandonment of the multipurpose survey of everyone in favor of focused inquiries which do one thing at a time but do it well—whether that be the devising of a forecasting instrument, the providing of programmatic counsel, the refining of biometric evidence, or the testing of sociological hypotheses.

My final judgment, then, in this critique of the national fertility surveys is that they are really not that good, but, on the other hand, they're really not that bad. Back to the old drafting board.

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